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War Makes the Regime:

Regional Rebellions and Political Militarization Worldwide

Abstract: War can make states, but can it also make regimes? This essay brings the growing literatures on authoritarianism and coups into conversation with the older research tradition analyzing the interplay between war and state formation. We offer a global empirical test of the argument that regional rebellions are especially likely to give rise to militarized authoritarian regimes. While the argument was initially developed in the context of Southeast Asia, the article deepens the original theory by furnishing a deductively grounded framework embedded in rational actor approaches in the coup and civil-military literatures. In support of our argument, quantitative tests confirm that regional rebellions make political militarization both more likely and more enduring, not simply in a single region, but more generally.

In February 1989, a group of military officers presented Sudanese Prime Minister Sadiq Al-Mahdi with an ultimatum: either give the military the means to achieve a battlefield victory against the secessionist regional rebellion in Sudan's south or end the conflict by political settlement. He was unable to do either. Four months later, he was ousted in a bloodless coup led by Brigadier Omar Al-Bashir (NYT 1989). The coup ushered in a military regime which has lasted 30 years and continues, despite Bashir's ouster in April 2019 in the face of popular mass mobilization.

The Sudanese case conforms to the central argument we make in this paper. Regional rebellions – where rebels seek separation or autonomy from the political center, not its conquest – are associated with transitions to militarized regimes. Building on Slater (2010, 2018), this paper furnishes a deductively grounded theory for this association between a distinctive type of war – *regional rebellions* – and one particular type of authoritarian regime: *militarized regimes*. To explain the link, we emphasize two causal mechanisms: *disalignment of preferences* between civilian and military leaders and the military's *autonomous operational experience* in fighting regional wars.

When subjecting this macro-association to empirical tests on a global dataset, we find strong empirical support for our argument that *regional rebellions stand out among all forms of war in making militarization more likely and more enduring*. Regional rebellions double the likelihood of the subsequent emergence of a military regime; and military regimes that experience a regional rebellion during their spell are 45 percent less likely to democratize. We also provide *illustrative qualitative* examples of our mechanisms in Asian and African cases of regime militarization. Taken together, this study establishes regional wars as a key causal variable for explaining the emergence and durability of one specific type of authoritarian regime.

We make several theoretical contributions. While the authoritarianism literature has made great strides in explaining the effects of different regime types on various outcomes (e.g. Chandra and Rudra 2015; Weeks 2012), scholars are still to systematically consider why different types of authoritarian regimes emerge in the first place. The literature on war, conversely, has predominantly sought to explain war occurrence, while war as a producer of political outcomes other than state formation (e.g. Tilly 1985; Thies 2004) has been neglected.

This paper brings together these disjoint literatures and establishes war as an independent variable for regime type outcomes. The study also refines the literature on civil-military relations and coups, which has exhibited a tendency to lump together domestic conflicts of all kinds as determinants of military interventions, invoking concepts such as "mass, organized, and politically violent opposition" (Svolik 2013, 767), "civil conflict" (Piplani and Talmadge 2015), or "instability" (Leon 2014, 382). Our argument conceptually separates armed from unarmed domestic conflicts and, among armed conflicts, argues that only one specific type – regional rebellions – increases the likelihood of militarized regimes. More generally, our conceptual emphasis on militarization – a term we derive from seminal texts on civil-military relations – highlights the possibility of military rule through means other than coups.

In the following sections, we outline how this paper relates to the coup literature, define the concept of militarization, and remark upon the surprising dearth of literature connecting types of war to types of political regimes. We then present our theoretical argument building and expanding on Slater (2010, 2018), elaborating on the main causal mechanisms with the help of illustrative case evidence. Our quantitative analysis then tests the argument's generalizability to the global population of countries. We conclude with a reflection on the broader relevance of our findings.

Coups and Militarization

Recent literature on coups has made significant strides in explaining the motivations of militaries to seize power, highlighting structural factors such as slow growth or lagging human development and window-of-opportunity events (e.g. weak electoral performance) as determinants (e.g. Hiroi and Omori 2013; Wig and Rod 2014). Military involvement in politics in this line of inquiry boils down to a singular event – the coup – commonly defined as an "illegal attempt […] to unseat the sitting executive" (Powell and Thyne 2011, 252).

While such a dichotomous conceptualization offers clear measurement advantages (Feaver 1999, 219), "coup-ism" problematically defines the most extreme event as the conceptual benchmark while neglecting other ways militaries exert political power (Croissant et al. 2010). The absence of a coup is not necessarily a sign of successful civilian control; rather, it may indicate that a politically powerful military has been granted influence and prerogatives *ex ante*, rendering a coup unnecessary (Feaver 1999, 218). Coups also frequently involve one military faction overthrowing another, rather than displacement of civilian by military power (Geddes, Frantz, and Wright 2014).

In contrast, foundational texts on civil-military relations called attention to the different degrees to which regimes can be *militarized*, that is, the extent to which the military exerts political power in an ongoing fashion. Finer's (1962) seminal study distinguishes four levels of military involvement, ranging from mere "displacement" of critical with pliant civilian leaders, to full-blown "supplantment" of civilian leadership through direct military takeover. Similarly, Stepan (1988) highlights a range of institutionalized prerogatives of Latin American militaries. As Brooks (2019) reminds us, civilian authority or, in Huntington's (1957) terms, "objective control" of

the military by civilian leaders can be compromised without coups. We recover this concept of militarization here.

Following this literature, militarization means that key decision-making power over policies unrelated to national security is usurped by the military. As a result, militarization necessarily entails an autocratic form of government as unelected military officials decide major non-military policies. How militaries exert power has taken different institutional shapes (e.g. high-powered national security councils, military juntas, key ministerial portfolios occupied by military officers), and the degree to which militarized autocracies have used political institutions (e.g. parties, parliaments) and allowed political participation has been equally variable.[1]

To make this empirical variation analytically manageable, we build on Finer's (1962, 4) concepts of "displacement" and "supplantment" and propose a corresponding distinction between two broad variants of militarization: direct and indirect. Militarization is *direct* when the chief executive is an active-duty officer,[2] who is *often but not necessarily* backed up by an institutionalized military decision-making body (e.g. a junta); it is *indirect* when a *civilian* leads the executive and the military wields political power from the background, for instance through a national

[1] This distinguishes our concept from the notion of praetorian regimes which Huntington (1968, 78-82) defines as necessarily under-institutionalized and which, understood as being at an intermediate stage of a linear modernization process, are consequently prone to instability and rupture. Some but by no means all militarized regimes can be praetorian.

[2] This does not include former soldiers who won in competitive, multi-party elections.

security council "advising" the government. Both types can be described as *militarized regimes*.[3]

Alternative conceptualizations of "military-led autocracies" (e.g. Geddes, Frantz, and Wright 2014) do *not* encompass indirect military rule behind a civilian executive, although such indirect militarization is frequently observed.[4] For instance, the 1957 constitution of Honduras granted the military expansive powers to influence state spending and policies (Ruhl 1996). Similarly, the National Security Council in Turkey historically allowed the military to influence, and if necessary force to abdicate, civilian governments (Narli 2000). The Pakistani military undermined the power of post-Zia civilian governments to exercise authority over key state personnel and policies (Shah 2014, 168). By using the concept of militarization, and distinguishing direct and indirect variants, we capture the fact that militaries can wield political power without supplanting civilian governments.

While regime militarization often begins with a coup, a coup does not always commence regime militarization. Coups can initiate democratic transitions (e.g. Marinov and Goemans 2014) or represent a military effort to replace one set of civilian leaders with another. Conversely, regime militarization may involve a gradual military effort to acquire power and exert it indirectly, or situations in which overburdened civilian rulers hand over control to militaries. Different from coups, regime militarization is (to varying degrees) an enduring feature of political systems. It shifts

[3] To avoid a disconnect from the large literature on military regimes, we use the term "military regime" and "militarized regime" interchangeably to designate both direct and indirect military rule.

[4] Their variations of military-led autocracies (Geddes, Frantz, and Wright 2014, 152) all imply an officer as head of the executive.

our focus to the question of how and why militaries *secure* and *sustain* political power over long periods of time.

The Paucity of Theory on Wars and Regimes

Most empirical research on wars has sought to explain their occurrence. But what about the political effects of war? As Kasza (1996, 370) noted nearly a quarter century ago: "every aspect of domestic politics has felt war's influence. It is time […] to give this pivotal phenomenon the attention it deserves." Yet our relative inattention to war as a determinant of politics persists. This is particularly true for the most studied macro-level outcome of all besides war itself: democratic and authoritarian regimes.

Instead, scholars have mostly analyzed the effects of interstate and, more recently, intrastate war on *state formation* (e.g. Thies 2004). The nexus between war and *regime formation* has received much less interest. Ertman's (1997) study is a telling example. Unlike most bellicist works, Ertman strives to explain both states and regimes in European political development. Yet war only helps explain *states* in Ertman's theory. To explain the rise of constitutionalist *regimes*, Ertman relies on a non-bellicist variable: the territorial vs. status basis of medieval representative assemblies.

When bellicist theory does discuss regime effects, findings have been inconsistent. Some literature points to the democratizing force of external war as it leads to elite-level negotiations over taxes and state-building (North and Weingast 1989; Tilly 1992). By contrast, Downing (1993) argues that the military revolution in early modern Europe pressed monarchs to disregard and disband parliaments. Beyond elite bargaining models, recent research suggests that high levels of popular mobilization during civil war can foster democratic inclusion after an armistice

(Huang 2017). Contrarily, invocations of "garrison states" (Lasswell 1941) imply that wars have authoritarian effects. Some authors argue that participation or military defeat in *inter*-state war make regime breakdown through a military coup more likely (Bueno de Mesquita et al. 1992; Finer 1962). Others have shown that inter-state war diminishes the likelihood of military overthrows (e.g. Piplani and Talmadge 2015). Desch (2001) and Staniland (2008) report that *intra*-state conflict makes coups more likely. Bell and Sudduth (2017) argue that the risk of coups increases as a result of the costs imposed on combatants. Our analysis builds on their welcome shift in focus towards internal warfare, while more precisely specifying the type of civil war most likely to trigger this dynamic.

In summary, the war literature has devoted little sustained interest to regime formation. Even where it has done so, it has characteristically approached regimes as a dichotomy between democracy and authoritarianism. This is insufficient in light of the institutional diversity across authoritarian regimes that comparative research during the last two decades has established. The key distinction to emerge from this new wave of research is the contrast between authoritarian regimes in which power rests in a ruling party and those in which it rests in the military – a recurrent variation which remains to be systematically explained. Building on Slater's (2018) recent work, this essay argues that a productive way to tackle this puzzle is by focusing on the gap in the literature on the macro-level political consequences of war.

War and Regime Militarization: Towards Micro-Foundations

The argument we test in this article is rooted in Cold War Southeast Asia (Slater 2010, 2018). Although virtually all Southeast Asian countries experienced severe internal warfare after World War II, they differed sharply in the predominant type of conflict.

Some newly born countries confronted armed challenges from groups seeking to *escape* the newly formed state, while others faced rebels aiming to *seize* the new capital. This difference was significant because, if violent conflict is to spark political development, it must unify a set of actors with sufficient resources to craft a new political order: either through revolutionary takeover or counterrevolutionary self-defense. Southeast Asia's experience with divergent forms of rebellion suggests that regional rebellions are the least likely type to inspire a cohesive counterrevolutionary coalition encompassing both military and civilian leaders.[5]

Our core purpose in this section is to decontextualize Slater's original theory and parlay the existing, inductively generated findings into a more deductively grounded theory inspired by rational-actor approaches. This results in refined and generalizable micro-foundations for the causal link between regional rebellions and militarization. We thereby embed the theory in literatures on coups and civil-military relations, specifically their emphasis on information asymmetries and strategic assessment, coordination problems, and corporate grievances.

We first elaborate a theory of two general mechanisms to explain the link between regional rebellions and regime militarization. We then present *illustrative* evidence from a variety of cases (starting in Southeast Asia) to elucidate how the stipulated mechanisms might work in practice. Our case material builds on a convenience sample and is only meant to serve as empirical illustrations, not a systematic test, of the mechanisms. As the causal mechanisms go beyond Slater (2018), the empirical material used to illustrate the mechanisms is new, although

[5] For the argument and further evidence that leftist, center-seeking rebellions spawned durable and civilianized authoritarian regimes in Southeast Asia, see Slater (2010, 2018) and Slater and Smith (2016).

drawn from some of the same cases. The main point of this section is to theoretically motivate our general expectation that regional wars are more likely to lead to militarization – the core hypothesis and empirical focus of this paper. A dedicated test of the causal mechanisms and their observable implications is beyond the scope of this current work.

We highlight two mechanisms – what we call the *disalignment* and the *operations* mechanism – through which the causal connection between regional rebellions and political militarization can unfold. These two mechanisms are conceptually separate and can occur independently of each other, although it is conceivable that they influence each other in specific cases. Although these mechanisms are not necessarily exhaustive, we consider them the most systematic ways in which regional rebellions shape regimes.

Disalignment

When war reshapes states and regimes, it does so by reshaping political coalitions. Of particular interest for our argument here, political militarization requires a political disalignment in which militaries become both divorced from and more distrustful of civilian leadership. The civil-military literature posits that militaries can reject the legitimacy of specific missions or develop divergent views about how and when to use force (Betts 1991; Feaver and Gelpi 2011).

We argue that regional rebellions are an especially likely candidate to disalign civilian and military elites in this fashion. Specifically, regional rebellions entail especially severe *information asymmetries* and hence prompt civilian and military leaders to develop *divergent beliefs* about the threat environment the regime is facing

and how this threat should be countered.[6] This can heighten the military leadership's corporate grievances, which are key motivations for military interventions (Nordlinger 1977; Thompson 1980).

Mostly huddled in major cities, civilian elites are predominantly focused on political activities and threats originating in and around the capital and other major population centers (Bates 1981; Waldner, Peterson, and Shoup 2017). What is more, given that taking rebellions into urban political centers is costly (Kalyvas 2006, 133) and the credibility of a signal increases with its costs, civilian elites should perceive rebel activity close to these centers, rather than far away from them, as highly threatening (Johnson and Thyne 2016). What is more, the effects of urban rebellion can often be directly observed by civilian elites – e.g. if certain quarters or roads become inaccessible – conveying battlefield information directly to politicians.[7] Better information on urban armed conflict also stems from the nature of urban counter-insurgency which typically involves civilian policing, surveillance, and social welfare measures (Ron 2003). This means civilian elites can rely on numerous civilian *and* military sources to form their beliefs about the threat environment.

By contrast, regional rebellions typically occur on the fringes of national territory, far away from the inner perimeter of civilian elites' threat perception.[8] Fighting is delegated to military elites who are expected to sacrifice themselves and

[6] Information asymmetry has long been identified as a key variable in violent conflict (e.g. Fearon 1995).

[7] On the importance of battlefield information in civil wars, see Cunningham (2006).

[8] On average, regional rebellions occur 413 kilometers away from the capital, while center-seeking rebellions occur at an average distance of 69 kilometers from the capital (data from Allansson, Melander, and Themnér 2017; Gleditsch et al. 2002).

their soldiers to hold the country together. In addition, crushing regional rebellions typically involves the use of crude military force while the involvement of civilian actors is much reduced (Schutte 2017). As a result of its specialization in dealing with such threats, coupled with the conflict's geographical remoteness, the military has a significant informational advantage over its civilian principals (Brooks 2008; Egorov and Sonin 2011; McMahon and Slantchev 2015).[9]

This information asymmetry can disalign civilian and military in two distinct ways: a "hawkish" scenario in which military leaders decide that civilian leaders provide insufficient support for fighting regional rebels, and a "dovish" scenario in which military elites want to settle for peace while civilian leaders prefer to continue fighting. In both scenarios, the key factor is disagreement over the conduct of the conflict, and the outcome as regards regime type – increased motivation of military elites to take over – is the same. The outcome as regards the approach to regional rebellion is different, depending on the type of scenario (hawkish or dovish). This, however, is not the main dependent variable of this paper. Both the hawkish and dovish scenarios entail a civil-military disalignment in which the regional rebellion creates extremely high stakes for the military – literally a matter of life and death – making them resent civilian leadership for conducting the war too carelessly and cluelessly.

HAWKISH SCENARIO

In the hawkish scenario, the military signals that threat levels are high and requests more resources to *crush* the regional rebellion. Civilian elites receive this as a noisy

[9] Information asymmetries also exist between senior officers, junior officers, and rank and file, but it can reasonably be assumed that higher-ranking officers can overcome these, albeit at a cost.

signal. At this point the "civil-military problematique" (Feaver 1996) kicks in: as civilian elites know that military elites have an incentive to exaggerate the threat to amplify their resources and, ultimately, political power, they will qualify or, worse, disregard the signal. In Feaver's (1996, 154) words, "the military can describe in some detail the nature of the threat posed by a particular enemy, but only the civilian can decide whether to feel threatened." The military, as a result, will feel increasingly reluctant to engage in and share information with joint civil-military assessment institutions, further exacerbating information asymmetries in a vicious circle (Brooks 2008, 45).

To the extent that civilian and military elites disalign over both how and how committedly to combat the regional rebellion, we can expect the military's frustration to rise and tolerance for civilian oversight to decay. Diverging threat perceptions can also lead to disagreements over strategy, doubts over civilian competence, and concerns over the military's status, which have all been shown to trigger military intervention (Desch 2001, 11–12; Finer 1962, 27; Brooks 2008). Failing a move by the rebels closer to urban centers, military elites cannot make their assessment of rebel strength more credible.[10]

The observable implications of this "hawkish" variant of disalignment are that civilian rulers have limited access to independent information about the regional conflict; they discount the military's threat description, motivated by suspicions that

[10] Conversely, this would explain why some regional rebellions, such as the Tamil Tiger insurrection in Sri Lanka, did not lead to militarization as the rebels carried the war to the capital, directly threatening civilian elites. Similarly, inter-state war should be indicative of a fairly serious threat that is common knowledge between military and civilian elites. This is particularly true as civilian elites face high political costs if they endure military defeat at the hands of other states (Chiozza and Goemans 2004).

the military exaggerates the threat strategically (which can indeed be the case), in turn frustrating military leaders and motivating political intervention. All this should be reflected in military and civilian strategic discussions. In practice these can be difficult to observe, though we descriptively illustrate much of the process in the Burmese case below.

DOVISH SCENARIO

In the alternative dovish scenario, the military concludes that the losses from fighting the regional rebellion are too high and an honorable negotiated settlement is needed to avoid pointless bloodshed. Meanwhile civilian elites, with no personal skin in the game, believe a decisive military victory is attainable. Military elites could in principle simply tell their civilian principals that the rebels cannot be beaten. Yet, in practice military elites will have incentives not to do so. As Goemans (2008, 777) points out, acceptance of defeat by the military frequently entails a reorganization of the military, "with significant implications for the careers and prospects of the officer corps." What is more, military elites themselves compete with other officers and thus know that the risk of rival brass coming forward and declaring that s/he will be capable of defeating the rebels is real (Brooks 2008, 50). Save for putting their career and position in the regime coalition at stake, military elites may therefore decide that the only way of ending the conflict is to take over control themselves.[11] Even in cases where military elites initially share their concerns with civilian rulers, extant research on strategic assessment suggests that, fearing that continued dissent will entail professional suicide, they will begin to withhold information, grow wary of participating in joint

[11] This is in line with Thyne's (2017) finding that coups carried out during civil wars actually decrease the duration of the conflict.

forums, and act more independently in the face of continuing disagreement with civilians (Brooks 2008, 5; 250). Ultimately, they may conclude that a fundamental change of strategy can only be achieved by forcefully sidelining civilian rulers.

The observable implications of this "dovish" variant of disalignment are that military leaders become convinced that a military solution is impossible; cease communicating this to civilian elites (or never communicate it in the first place) out of fears of punishment and demotion, and eventually take this as a reason to intervene. This process is illustrated in the Thai example below.

Empirical Illustrations

Burma's 1962 coup is a useful example of the hawkish scenario unfolding under conditions of information asymmetry. In April 1960, Prime Minister U Nu was reelected after handing over power to a military caretaker government under General Ne Win in 1958. While rebel activity throughout Burma had markedly declined between 1958-1960, Nu's return to office in 1960 coincided with a rapid resurgence of rebel militancy in Shan state. Simultaneously, the Karen National Defence Organization (KNDO) intensified their military campaign in apparent coordination with Shan rebels. Additionally, with alleged weapons supply from the US, remnants of the Chinese Kuomintang fighters present on Burmese territory joined forces with Karen and Shan rebels in early 1961 (Trager 1963, 316–317). The resurgence of rebel activity was viewed extremely anxiously by the army (Smith 1991, 185). In declassified CIA reports from January 1961, chief of staff Ne Win is quoted saying that the situation has reached "serious proportions" (CIA 1961a).

In stark contrast, Nu seemed to be in denial, rejecting the risk assessment put forward by the army. The split of the AFPFL ruling party in April 1958 decimated local party infrastructure and left Nu with very limited sources of independent civilian

information on developments in the countryside (Butwell and von der Mehden 1960). In August 1960, he stated that "there's calm, peace, and lack of tension in the country, no one can deny it" (New York Times 1960). In fact, he blamed rebel "luck" and, paradoxically, the army's success in forcing rebels out of hideouts for the worsening security situation and denied that the situation was "as bad as 1949-50" – the worst year of the insurrection (Smith 1991, 189–190; Trager 1963, 317–318). This caused an outcry in the Burmese press, which demanded a vigorous increase in the size of the armed forces and more military spending (Trager 1963, 317–318).

Yet, this would have gone against Nu's goal of pushing back the influence of the army (CIA 1960b, 1960a). Nu had previously accused the army of favoring the rival Stable AFPFL party before the election (Bigelow 1960, 71). Accepting the military's risk assessment and boosting the army's resources and standing was thus not an option. Holding out vigorously against army budget requests (Callahan 2005, 6, fn. 42), Nu diverted troops to the Chinese border for survey and demarcation missions, reorganized the army and gave the civilian Ministry of Defense tighter control over all military activities (Callahan 2005, 202; Chang 1969, 826). The CIA noted in May 1961 that the war effort against ethnic insurgencies had visibly declined (CIA 1961c). With Nu's refusal to acknowledge the heightened security threat, patience within the military evaporated (CIA 1961b), culminating in a military coup led by Ne Win in March 1962. In one of their first post-coup statements, the army promised "to stamp out" the resurgent insurgencies (cited in Trager 1963, 322).

Such hawkish scenarios differ meaningfully from the dovish scenario. The latter is well illustrated by the coup against Thai Prime Minister Thaksin Shinawatra in September 2006. Here, the military's frustration with Thaksin's heavy-handed handling of the insurgency in the country's South was a key driver (Funston 2009;

McCargo 2007; Pongsudhirak 2006; Sheridan 2006). Thaksin responded to early incidents of violence in 2001 by berating the army in the South, threatening that "transfers will be ordered for those who are inefficient […] They won't be around here" (McCargo 2006, 47). At the same time, and despite a worsening security situation from mid-2001, Thaksin disbanded the military-led Southern Border Provinces Administration Center (SBPAC) and the joint civilian-police-military task force, CPM 43, and diverted control from the military to the police, with which he enjoyed closer political ties (Croissant 2007, 11; International Crisis Group 2005a, 33; McCargo 2006, 48).

As the conflict worsened from 2002 onwards, Thaksin ordered the police and army to respond with full force (Pathmanand 2006). Martial law was introduced to three Southern provinces in January 2004 and troop size increased radically, reaching 20,000 by October 2005 (ICG 2005a, 19). This repressive strategy resulted in a rapidly rising death toll – over 1,000 between 2004 and 2006 (Pathmanand 2006, 74) – and increasing government abuses, such as the suffocation of 85 Muslim men in detention in Bak Tai (ICG 2005a, ii). High-ranking military officers were deeply unhappy about this incident, which they saw as tarnishing their reputation (Pathmanand 2006, 82). The killing and public display of mutilated Thai marines in September 2005 left a profound psychological impact on troops in the South (ICG 2005b, 19). While military casualties were as high as police casualties (Chalk 2008, 10), decision-making had been placed in the hands of the police, with profound repercussions for strategic assessment and information sharing with civilian authorities.

While the police kept feeding Thaksin intelligence blaming organized criminals for the resurgent violence and engaged in extra-judicial killings of former army informants (ICG 2005a, 34), the military command in the South gradually

stopped sharing information with civilian leaders in Bangkok. According to General Vinai Pattiyakul, secretary-general of the National Security Council, "[military] intelligence agencies had discovered the establishment of separatist insurgency networks since 2002 but no one dared tell the prime minister" (cited in Pathmanand 2006, 79). Thaksin's actions gradually established a belief among the army high command that, as a former police lieutenant colonel, Thaksin would rather listen to the police than the military and that speaking out would only add one's name to the list of inefficient officers to be rotated out (McCargo 2006, 49). Even Thaksin's cousin, General Chaiyasit Shinawatra, who briefly assumed the role as commander-in-chief between 2003 and 2004, quickly came to believe that Thaksin favored the police (McCargo 2006, 54) – he was only one of five commanders-in-chief in an increasingly arbitrary pattern of punishment transfers (ICG 2007, 1; McCargo 2006, 53–54).

The impact of this breakdown of information sharing on the government's ability to manage the rebellion was devastating. "A source familiar with Thaksin's intelligence-gathering capabilities argued that the prime minister lacked accurate information and analysis of developments in the South. By early 2004, the government had virtually no reliable human intelligence from the ground" and "[i]t seemed abundantly clear that no one in power really understood what was happening around the southern border" (McCargo 2006, 51;54). US embassy cables repeatedly mention a phenomenon of stove-piping (often erroneous) information from local agencies to Bangkok (Wikileaks 2005, 2006a). At the same time, the army began to increasingly act independently of Thaksin, with soldiers sharing information only with senior army officials (ICG 2007, 1;16; Wikileaks 2006b, 3). Reestablished joint command structures in 2004 were in effect boycotted by the police and did not repair the

informational breakdown (Pathmanand 2006, 82). Additionally, the Southern army command and indeed officers in central command increasingly turned to the Privy Council and the King to voice their concerns. Harsh criticism of Thaksin's strategy by the Council's president and vice-president in 2004 can thus be seen as expression of military frustration (ICG 2005b, 2; McCargo 2006, 62–63). Following a meeting between Malaysian Prime Minister Mahathir and Thai King Bhumibol, the army displayed their keenness to end the conflict by engaging in secret negotiations with the rebels under Mahathir's auspices, an approach rejected by Thaksin (Harish and Liow 2007, 172; ICG 2007, 2).

Emboldened by his election landslide in 2005, Thaksin "supplemented martial law in the South with an emergency decree in July 2005[, which] transferred the responsibility for dealing with the insurgency […] to the prime minister's office" (Storey 2008, 42). Meanwhile, Thaksin paid lip service to a more conciliatory approach by establishing a national reconciliation committee, while ignoring the dovish request of his commander-in-chief Sonthi for a negotiated solution (Harish and Liow 2007, 170). Instead, he publicly chided Sonthi in June 2006 for failing to prevent a wave of bombings (ICG 2007, 1). Thus, prior to the coup in September 2006, it was clear that "[t]he Army were deeply unhappy that they were being blamed for a problem they believed Thaksin and the police […] had largely created" (McCargo 2007, 14). Days before the coup, General Sonthi declared: "Soldiers all of us, privates and generals, are risking our lives every day. I don't want to see […] politicians putting all the blame on field officers" (Human Rights Watch 2007).

Operations

The second mechanism linking regional rebellions to militarization relies on the particular operational experiences that militaries acquire in fighting regional rebellions. They tend to lead to 1) a stronger esprit de corps and social cohesion of the military; 2) improved coordination capacity across military branches; and 3) increases in the military's non-coercive organizational and bureaucratic capacity. We derive these effects from existing civil-military literature and illustrate them briefly with the cases of the Mauritanian and Indonesian military takeovers.

As we mention above, militaries are not the only organizations that regimes have at their disposal to battle rebels. Institutions for civilian policing and bureaucratic governance can also be enlisted "to win hearts and minds." Yet the remoteness and specificities of the terrain rebels hide in can greatly complicate the involvement of civilian actors. This means that militaries are often given a free hand, as much by default as by design, to suppress regional rebellions. Such campaigns tend to rely on military force alone, in contrast to joint civilian-military interventions prevalent in urban counterinsurgency (Ron 2003). This is evidenced by Schutte's (2017) finding that the use of crude military force without civilian involvement increases with distance from the capital city. This *shared operational experience devoid of civilian support* can significantly enhance the military's esprit de corps and increase its cohesion as a status group. Both esprit de corps and symbolic status of militaries have been associated with a higher inclination to engage in politics (Finer 1962, xv;10; Nordlinger 1977, 65). Note that this process operates, in principle, independent of disalignment: Higher cohesion and social status resulting from the fighting of regional wars do not automatically lead to disagreement over the conduct of operations and vice-versa – regional wars just tend to trigger both in parallel.

The observable implications of this process are straightforward: Separate, autonomous operations during regional rebellions boost the military's social cohesion and elevate its social status over time. This, in turn, can facilitate political involvement. This is what we observe in the example of Mauritania where the military's social status and collective identity as a leading national institution strengthened considerably throughout the conflict with Western Saharan rebels from 1975. While the military was embryonic when President Ould Daddah started the conflict, it gained significant combat experience and esprit de corps over the coming years, preparing the top brass for the 1978 takeover of power from a civilian leader perceived as detached and strategically inept (Pazzanita 1996, 47).[12]

The coup literature further points to the importance of *coordination capacity* in making military takeovers possible (Pilster and Böhmelt 2011; Singh 2014). The conduct of regional military operations typically leads to the improvement of exactly such capacity between different military units as it often relies on different branches of the military working closely together. This is, again, a process that in principle is clearly observable in terms of increased cooperation of military branches over time, the more frequent interaction of senior officers it brings about, and the military elite's subsequent use of such capacity to coordinate collective political action. For example, while Indonesia's army had been rife with competition between the army, navy, and

[12] Although less well documented, the Mauritanian case also illustrates the information asymmetry aspect of the disalignment mechanism. President Ould Daddah drastically underestimated the resistance from Sahrawi independence fighters (Clausen 1982, 40). The eventual military takeover was led by army chief of staff Mustafa Ould Salek, whose position "put him in a unique position to assess the increasingly unwinnable Western Saharan war" (Pazzanita 1996, 202) – information that he did not manage to credibly convey to the president, who insisted on continuing the high-cost conflict.

air force (Kingsbury 2003, 141), fighting rebels in Indonesia (1957-61) involved close cooperation of the country's air force, a sea blockade carried out by Indonesia's navy, and the deployment of infantry units, especially paratroopers (Doeppers 1972; Feith and Lev 1963). Institutionally this led to the establishment of the Central War Administration, which coordinated military efforts during the state of emergency. Such cooperation leads to a better exchange on preferences within the military and better information on the likely support for military involvement in politics. While we would expect junior officers and rank and file to be busy fighting the rebellion, elite officers, having improved their coordination capacity, can potentially rely on troops not deployed in the restive region to overthrow the civilian government. This is supported in our data as all military takeovers following regional rebellions but one (the Comoros in 1998) were carried out by the military leadership, often led by chiefs of staff.

Finally, waging war against regional rebels *heightens the organizational and bureaucratic capacity* of the army. The war effort is typically accompanied by processes of professionalization and expansion of the military organization. Importantly, this expansion often means that the army branches out into civilian realms of government and production. This is directly observable in the widening of non-core operations and attendant organizational changes of the military, which in turn motivate and facilitate political involvement and takeover by the military. As Eck (2015, 232) remarks on the case of Burma, "[i]n 1952, Ne Win reorganized and expanded the resources dedicated to officer training [and] the armed forces ventured into business by setting up the Defense Services Institute (DSI) which, by 1960, was running banks, shipping lines, and the largest import–export operation in the country […] this organization was established to ensure that armed units were supplied." Similarly, Lev

(1964, 351) shows that martial law declared on the Indonesian islands of Sulawesi and Sumatra made the military the *de facto* rulers of the islands, subordinating the civilian administration to military orders. These processes of organizational and bureaucratic upgrading bestow precious managerial capacities upon military elites, putting them on par with civilian elites. As the Indonesian chief of staff, Nasution, declared in front of military cadets in 1958, the army "must be given an opportunity to make use of their skills in the Cabinet, the National Council, the National Planning Board, diplomatic posts, and elsewhere in government" (Lev 1964, 359). As a result of their "unconventional" activities the army would "feel they have as much to contribute as civilian leaders to the nation's development, perhaps more" (Lev 1964, 364).

In sum, fighting regional rebels tends to augment the military's esprit de corps, coordination, and organizational capacity, giving military brass more confidence in their abilities, including in the civilian realm. All three factors are progressively strengthened as fighting becomes more intense and protracted. As Maung (1969, 231) points out in the case of Burma, with the civilian government confined to major towns, the long duration of the rebellion forced officers to acquire leadership skills of civilian administrators. Ultimately, their improved skills in governance can lead the military to believe that they are not only better placed to decide how to fight the war, but also – when disagreeing with civilian leaders – better able to run the country, either directly or indirectly.

The disalignment and operations mechanisms provide conceptually separate pathways to militarization, although they may influence each other. Heightened self-esteem and capacity can lead to more coherent interests that are more clearly disaligned with civilian leadership. Disalignment itself might also trigger greater efforts to operate autonomously. While such mutual causation is conceivable, the

mechanisms can operate in parallel (as in our illustrative case material). They can also operate without each other: For example, operational capacities might remain low due to political or social reasons (such as the class or communal cleavages of many militaries), yet military brass can disagree with civilian leaders about strategy in a regional conflict.[13]

Testing external validity: Quantitative analysis

We now subject our theory to quantitative tests to establish whether the association between regional rebellion and militarization holds in a global context. Our theoretical discussion thus far implies the following hypotheses:

> H1: Countries that experience regional rebellions are more likely to become militarized authoritarian regimes.
>
> H2: More intense regional conflicts should lead to greater militarization of the regime.

We test these predictions by fitting two separate sets of regressions for the emergence of military regimes (H1) and for their extent of militarization (H2). In the Online Appendix, we also test a third hypothesis, namely that military rulers emerging from regional rebellions cling to power longer as their motivation and capacity to do so are stronger due to preceding preference disalignment and operational experience, resulting in a strong preference against the return to civilian rule. Our findings are supportive of this claim, albeit less unequivocally than for H1 and H2.

[13] We thank an anonymous reviewer for encouraging us to spell this potential interaction out more clearly.

Data

We use Svolik's (2012) dataset on authoritarian regimes to measure the variable of primary interest – regime militarization. We prefer Svolik's data over alternative datasets for three reasons: First, tit has the widest coverage in terms of countries, yielding over 8,500 observations compared to 7,700 for the alternative Geddes, Wright, and Frantz (2014) data. Second, the dataset allows for an unambiguous coding of military regimes which avoids the problem of manifold hybrids as in the Geddes et al. data. Third, and critically, the data distinguishes different degrees of military involvement in politics, enabling us to capture militarization below the level of a full-fledged military takeover.

We code the variable *Military regime* for all autocracies as 1 if military involvement in politics is either indirect, i.e., "the head of the executive is a civilian executive but the military intervenes in government policies unrelated to national security" (Svolik 2012, codebook), or military involvement is direct. In the latter case, military rule can be institutionalized in a corporate fashion (e.g. the classical junta) or be personal with the military ruling through a personal leader without institutionalization (Slater 2003). Non-military regimes are coded 0 and comprise both civilian autocracies and democracies. We also construct a categorical measure of military regimes (*Military regime categorical*) to test our militarization hypothesis (H2). The variable takes the value of 0 for non-military regimes, 1 for indirect military rule, and 2 for direct military rule.[14]

Data for our main independent variables – regional rebellions and, as contrast cases, center-seeking rebellions – are taken from the UCDP/PRIO dataset on armed

[14] Ideally, we would measure militarization on a continuum, but we are limited by lack of available data.

conflicts (Allansson, Melander, and Themnér 2017; Gleditsch et al. 2002). We define a rebellion as regional if the incompatibility underlying the conflict is over a territory. Center-seeking rebellions are characterized by incompatibility over government. As the total effect of rebellion is unlikely to unfold in one year only, we model the effect of rebellion with an exponential decay function. Specifically, the variable *Regional rebellion (decay)* takes the value of 1 whenever a regional rebellion is ongoing after which it decays exponentially with a half-life of one year. *Center-seeking rebellion (decay)* is defined equivalently for rebellions with incompatibility over government.[15] To measure the intensity of rebellions, we also create an additional count variable *Duration of regional/center-seeking rebellion* measuring the duration of an ongoing rebellion in years. We prefer this measure over alternative indicators, such as casualty numbers or the economic cost of destruction, which are frequently unreliable.[16]

Although Piplani and Talmadge (2015) have argued that inter-state conflict makes regime military overthrows less likely, we err on the side of caution by including a control variable for the incidence of inter-state wars in a given year (Allansson, Melander, and Themnér 2017; Gleditsch et al. 2002).[17] In addition, we

[15] Our results are robust when using alternative half-lives (2, 3, and 5 years). We also obtain very similar, albeit slightly weaker, results when using a dummy version of regional and center-seeking rebellion which constrains the total effect of rebellion to unfold in one year (see Online Appendix, Tables A7-A8).

[16] Five percent of center-seeking and three percent of regional rebellions in our dataset received support from external actors (data from Cunningham, Gleditsch, and Salehyan 2013). Unsurprisingly, tests show that it is the unsupported regional rebellions which drive our finding. External support as a control variable is insignificant and leaves our findings unchanged. Results available upon request.

[17] We also experimented with a decaying and duration version of *War*. There was no improvement in model fit and substantive findings are identical. Results available upon request.

include a battery of socioeconomic and political controls in our baseline models. As the democratization literature suggests a strong link between a country's wealth and regime type (Boix and Stokes 2003; Przeworski and Limongi 1997), *GDP p.c. (log)* measures the logged per capita GDP in constant 2005 USD (taken from Bolt and van Zanden 2014).[18] We also include *Population size (log)* using data from the World Development Indicators (World Bank 2017).

Establishing military rule in politically open systems might be harder; conversely, transition to democracy might be easier. We therefore include the variable *Polity* to approximate a country's degree of political openness using the standard Polity 2 indicator (Marshall, Gurr, and Jaggers 2010). The variable *Military capabilities (cube root)* is based on the widely-used composite index of national capability and captures, amongst others, material benefits directed towards the military (Singer et al. 1972, v5.0). Transformation using a cube root function yields the closest approximation to a normal distribution.

The coup literature highlights that civil unrest increases the risk of a military takeover (e.g. Johnson and Thyne 2016). Based on the Banks (2011) dataset, we therefore include the variable *Unrest (log)* in the regressions, which sums up and log-transforms the number of riots, demonstrations, and general strikes occurring in each country-year. Conflicts leading to military rule might also be more likely in religiously fractionalized societies. We therefore include a measure of *Religious fractionalization* (Alesina et al. 2003). Finally, we control for regional fixed effects by including region

[18] GDP data on missing countries are taken from Penn World Tables (Heston, Summers, and Aten 2012). We only use one data source per country. Our findings are nearly identical when using Bolt and van Zanden data only.

dummies.[19] Taking into account the effects of listwise deletion, our main models generally comprise the time period from 1961 until 2008.[20]

Modelling military regime emergence

Our first hypothesis posits that regional rebellions make the subsequent emergence of a military regime more likely. This calls for a model of military regime *onset*, which we model as a Markov transition model. The model allows us to separate the effect of rebellions on regime onset from the effect on regime durability by conditioning the model on whether or not an event occurred in the previous year (Beck 2008, 490). In our case, this means that we condition the model on the absence of a military regime in the previous period. Modelling event onset this way has been shown to be superior to alternative strategies, such as setting ongoing years to zero or missing (McGrath 2015). We opt for the widely-used binary time-series cross-sectional (BTSCS) regression model as our statistical workhorse. The model takes the following specification:

$$\Pr\left(Y_{i,t} = 1 \middle| Y_{i,t-1} = 0\right) = Logit(\alpha_0 + \beta_1 R_{i,t-1} + \beta_2 X_{i,t-1} + \delta_3 + \beta_4 \vartheta_{i,t} + \mu_{i,t}), \quad (1)$$

where α designates the constant, R our measure of regional rebellions, X a vector of covariates, and δ regional fixed effects. To deal with the issue of repeated events, we add the event counter ϑ measuring the number of previous transitions to a military regime (Beck, Katz, and Tucker 1998). We add μ, a vector of cubic time polynomials to account for duration dependence (Carter and Signorino 2010).[21] All

[19] Descriptive statistics of all variables can be found in the Online Appendix, Table A1.

[20] Descriptive patterns for H1 and H2 are shown in the Online Appendix, Figure A1 and A2.

[21] The measure of duration time captures the time spent until the emergence of a military regime.

variables except for regional fixed effects and the event counter are lagged by one year to ensure the correct order of temporal dependence.

One characteristic of the above model is that, by pooling the data, it does not clearly separate between-country and within-country effects. This is potentially problematic as our findings could be driven by unit-level (country) effects which the proposed specification fails to account for. We therefore also estimate a more conservative conditional logit model which controls for country-specific effects by conditioning on the number of successes in a group (Chamberlain 1980).[22] Conditioning on successes, in turn, means that all countries that did not experience a military regime are dropped from the analysis, and time-invariant variables cannot be estimated.[23]

The results of our pooled and fixed effects transition models are summarized in Table 1. Columns 1-4 report the pooled logit model while successively adding control variables; column 5 reports the results of the conditional logit model. Taken together, the results provide strong support for our claim (H1) that regional rebellions are associated with an increased likelihood of military regime emergence. Looking at the pooled logit model (columns 1-4), the coefficients of *Regional rebellion* are positive and highly statistically significant throughout. This contrasts with *Center-seeking rebellion* for which the model does not find a statistically significant effect. *War*, another potential confounder of the effect of regional rebellion, is also

[22] Simply adding country dummies into the logit regression would be problematic due to the incidental parameters problem (Beck 2015).

[23] To ensure that dropping countries does not bias our results, we test an alternative fixed effects specification using the Mundlak-Chamberlain approach (Chamberlain 1982; Mundlak 1978) in the Online Appendix (Tables A5-A6).

insignificant, suggesting that the cohesion or political ambition of military elites primarily, if not exclusively, stems from regional rebellions. Adding further controls diminishes the effect of regional rebellion only slightly. Our findings on control variables are mostly in line with theoretical expectations: Military takeovers are less likely in politically more open countries (*Polity*) and whenever greater resources are directed toward the military (*Mil. Capabilities*), while *Unrest* facilitates military power grabs. Reassuringly, R*egional rebellion* retains its strong positive effect in the conditional logit model.[24]

To illustrate the substantive effect of regional rebellions we look at the percentage increase in the likelihood of a military regime emergence relative to the average baseline probability. In the pooled model (column 1), the average annual probability of a transition to a military regime is 1.78 percent. A regional rebellion increases the likelihood on average by 1.66 percentage points, representing a remarkable 93 percent increase. In the case of the conditional logit model, the average increase is even bigger. Expressed in terms of odds ratios, the coefficient of *Regional rebellion* in the conditional logit is 7.8, meaning the odds of military regime emergence are nearly 8 times higher when there is preceding regional conflict. [25] Given our measurement of *Regional rebellion* as decaying over time, we also illustrate the temporal decline in the probability of military regime emergence following a regional rebellion. This is done in Figure 1. Following a significant increase in the probability of a transition toward military rule in period *t*, the probability gradually declines in

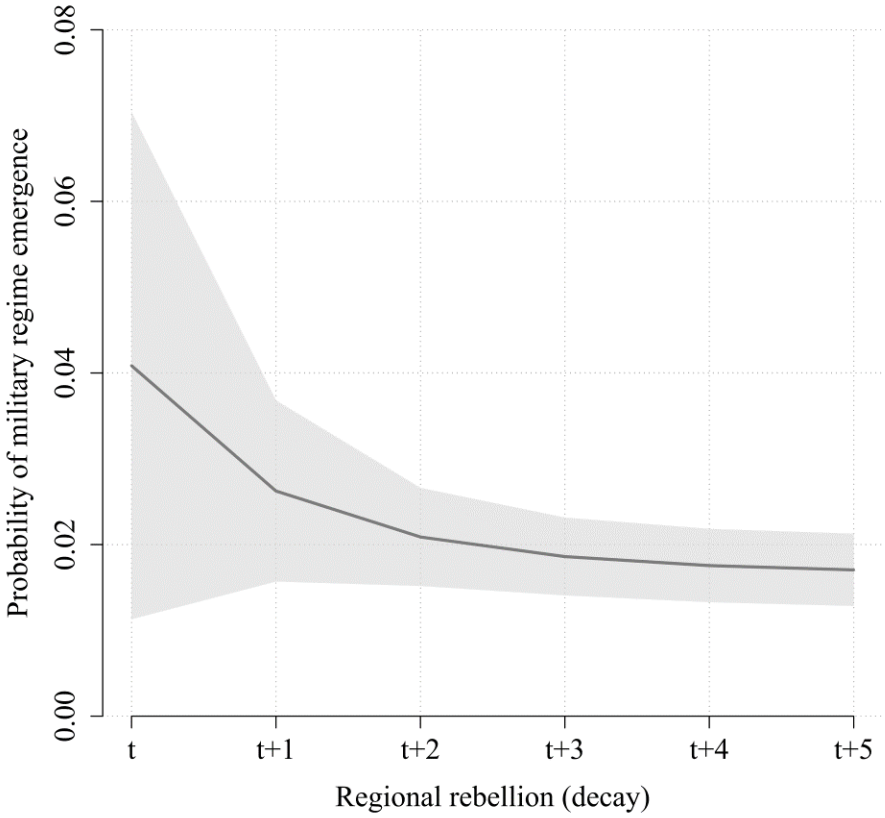
[24] Separation plots (Greenhill, Ward, and Sacks 2011) shown in the Online Appendix attest a reasonably good model fit.

[25] Unfortunately, it is not possibility to derive predicted probabilities from conditional logic models.

periods t_1 to t_4 until returning to levels around the baseline probability of about 1.8 percent in t_5.

As emphasized in the theory section, the information asymmetries between military and civilian political elites result from the remoteness of regional rebellions from urban centers. An observable implication of this is that regional rebellions should make military regime emergence more likely the farther they are away from the capital. This prediction receives strong support in data, as shown in the Online Appendix (Table A10 and Figure A6). This finding contrasts with Bell and Sudduth's (2017) finding that coups during civil wars become increasingly likely the closer the war moves to the capital. While some differences in the models might be expected due to the different dependent variables (coups vs. militarized regimes), we suspect that their sample restriction to only cases with ongoing civil wars (1441) accounts for most of the difference.

Figure 1: Effect of regional rebellion on military regime emergence



Note: Marginal effects derived from Table 1, column 1. *Regional rebellion* set at 1, 0.5, 0.25, 0.125, and 0.0625 according to decay function. All other covariates set at their observed values (Hanmer and Ozan Kalkan 2013). All figure layouts are based on Bischof (2017).

Table 1: Regional rebellions and military regime emergence

	(1)	(2)	(3)	(4)	(5)
Regional rebellion (decay) t-1	0.998	1.010	0.892	0.858	2.057
	(0.444)**	(0.440)**	(0.434)**	(0.417)**	(0.982)**
Center-seeking rebellion (decay) t-1	0.386	0.388	0.295	0.261	1.143
	(0.363)	(0.364)	(0.346)	(0.351)	(0.734)
GDP p.c. (log) t-1	-0.159	-0.160	-0.172	0.009	-0.850
	(0.238)	(0.238)	(0.234)	(0.265)	(0.856)
Population size (log) t-1	0.042	0.044	-0.007	0.368	2.672
	(0.128)	(0.125)	(0.124)	(0.215)*	(2.087)
Polity t-1	-0.127	-0.127	-0.142	-0.153	-0.408
	(0.031)***	(0.031)***	(0.032)***	(0.035)***	(0.096)***
Previous failures	0.196	0.198	0.167	0.127	-1.055
	(0.213)	(0.214)	(0.237)	(0.250)	(0.978)
War (dummy) t-1		-0.143	0.041	0.373	0.400
		(0.774)	(0.751)	(0.741)	(1.050)
Unrest (log) t-1			0.480	0.515	-0.097
			(0.151)***	(0.154)***	(0.304)
Rel. fractionalization t-1			-1.574	-1.687	
			(0.693)**	(0.746)**	
Mil. capabilities (cube root) t-1				-10.101	-18.464
				(5.002)**	(32.296)
NxT	4,726	4,726	4,631	4,630	1,301
Time polynomials	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-4. Conditional logit model in column 5. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Modelling regime militarization

We use multinomial logistic regression to test our second hypothesis on regime militarization.[26] The dependent variable in this model is *Military regime categorical* which comprises three different categories: 0 for non-military regimes, 1 for indirect, and 2 for direct military rule. The intensity of regional rebellions is captured by our duration indicator *Duration of regional rebellion*. The multinomial logit takes the following functional form:

$$\Pr(Y_{i,t} = Mil.regime\ categorical) = Logit(\alpha_0 + \beta_1 D_{i,t-1} + \beta_2 X_{i,t-1} + \beta_4 \vartheta_{i,t} + \mu_{i,t}), \quad (2)$$

where α designates the constant, *D* our measure of rebellion duration, *X* a vector of covariates, the event counter ϑ,[27] and μ, a vector of cubic time polynomials.[28] As before, all variables save for the event counter are lagged by one year. Note that we do not condition the model on the absence of a military regime in the previous year as we did in the case of the Markov transition model. This choice reflects the fact that further militarization can occur even after the regime has transitioned to a military regime. That said, we also run the model on the subset of military regimes only to test if the intensity of rebellion leads to further militarization after the transition to military rule has occurred, effectively making the multinomial logit a choice model between indirect and direct military rule.

Table 2 displays the results predicting *Military regime categorical*. Columns 1-4 report the regression coefficients for the unrestricted sample of military and non-military regimes combined. Column 5 presents the results for military regimes only. The base category in the first four models is non-military regimes. In model 5, the base category is indirect military rule.

[26] We give preference to multinomial over ordinal regression as the assumption of proportional odds in the ordinal regression model are not met. That said, results using ordinal logit are nearly identical.

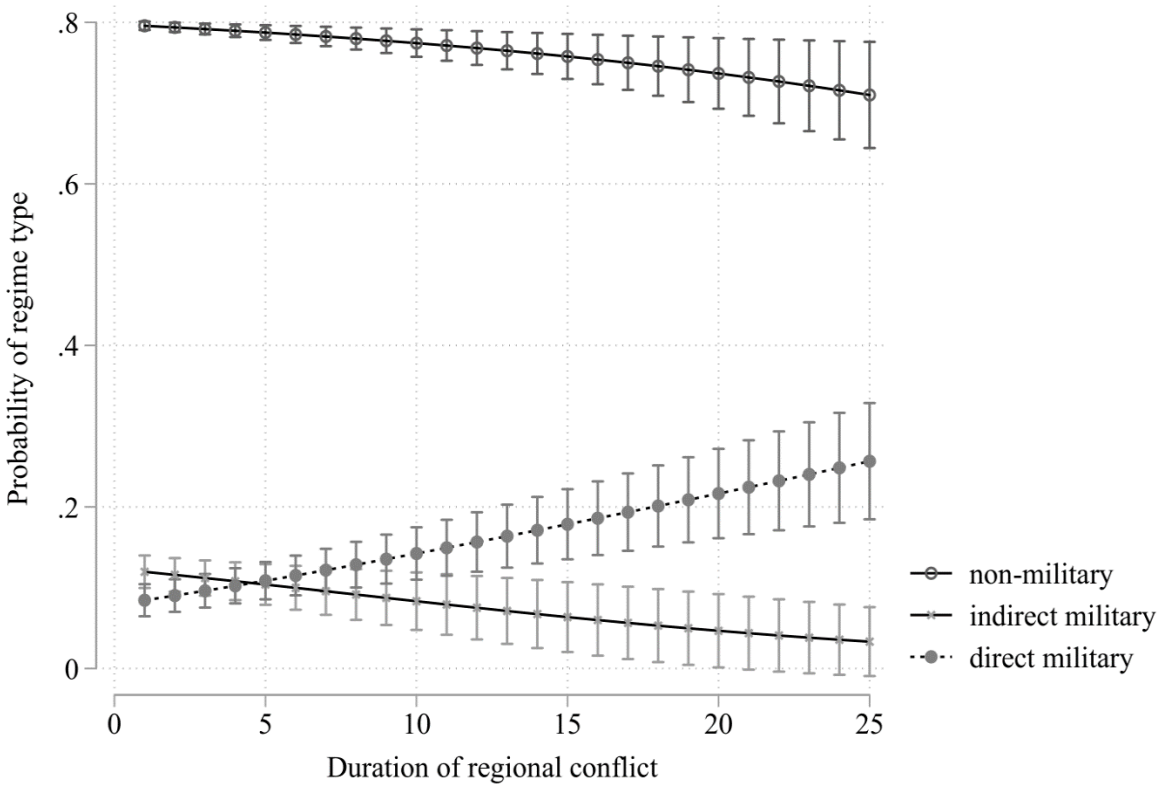
[27] An event is defined as any change in the dependent variable. Duration time thus means the time elapsed until such a change occurs.

[28] For the model to converge we had to refrain from using regional fixed effects.

As before, we add controls successively in columns 1-4. The findings provide substantive evidence for our claim that regional rebellions militarize regimes by pushing regimes more towards direct military rule.

We illustrate the size of the effect for all regimes combined in Figure 2 and for the subset of military regimes only in Figure 3. Both figures plot the effect of our categorical variable for different durations of regional conflict and illustrate the increasing probability of direct military rule at the expense of both indirect and no military rule. [29] In the two cases, direct military rule becomes significantly more likely than indirect rule the longer regional conflicts endure.

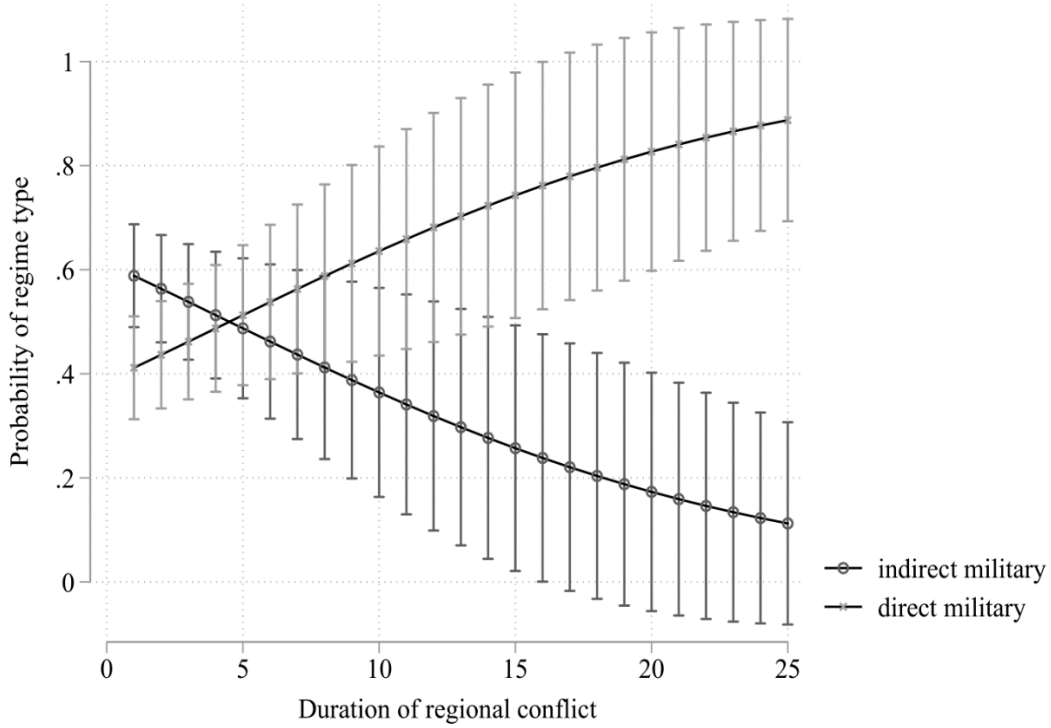
Figure 2: Effect of regional rebellion duration on militarization (all regimes)



[29] 0 to 25 years of conflict correspond to the 1st and 99th percentile in our regression sample.

Note: Marginal effects derived from Table 2, column 4. All other covariates set at their observed values (Hanmer and Ozan Kalkan 2013).

Figure 3: Effect of regional rebellion duration on militarization (military regimes only)



Note: Marginal effects derived from Table 2, column 5. All other covariates set at their observed values (Hanmer and Ozan Kalkan 2013).

Table 2: Regional rebellions and regime militarization

		(1)	(2)	(3)	(4)	(5)
Indirect Military	Duration of regional rebellion t-1	0.010	0.012	0.013	0.015	
		(0.044)	(0.042)	(0.041)	(0.041)	
	Duration of center-seeking rebellion t-1	-0.017	-0.018	-0.016	-0.018	
		(0.032)	(0.031)	(0.031)	(0.031)	
	GDP p.c. (log) t-1	-0.214	-0.213	-0.132	-0.068	
		(0.157)	(0.157)	(0.153)	(0.175)	
	Population size (log) t-1	-0.057	-0.055	-0.110	-0.012	
		(0.128)	(0.128)	(0.125)	(0.279)	
	Polity t-1	-0.182	-0.183	-0.189	-0.189	
		(0.026)***	(0.026)***	(0.025)***	(0.025)***	
	Previous failures	0.023	0.024	0.020	0.019	
		(0.010)**	(0.009)**	(0.010)**	(0.010)*	
	War (dummy) t-1		-0.125	-0.535	0.019	
			(0.652)	(0.532)	(0.684)	
	Unrest (log) t-1			0.036	0.051	
				(0.166)	(0.166)	
	Rel. fractionalization t-1			0.480	0.500	
				(0.588)	(0.588)	
	Mil. capabilities (cube root) t-1				-2.790	
					(6.399)	
Direct Military	Duration of regional rebellion t-1	0.110	0.114	0.118	0.120	0.116
		(0.030)***	(0.028)***	(0.026)***	(0.027)***	(0.050)**
	Duration of center-seeking rebellion t-1	-0.131	-0.134	-0.146	-0.150	-0.144
		(0.029)***	(0.030)***	(0.029)***	(0.029)***	(0.051)***
	GDP p.c. (log) t-1	-0.198	-0.198	-0.318	-0.150	-0.044
		(0.176)	(0.176)	(0.175)*	(0.251)	(0.394)
	Population size (log) t-1	0.263	0.273	0.227	0.500	0.553
		(0.103)**	(0.104)***	(0.098)**	(0.289)*	(0.542)
	Polity t-1	-0.180	-0.182	-0.189	-0.191	0.002

	(0.023)***	(0.023)***	(0.022)***	(0.023)***	(0.043)
Previous failures	0.020	0.020	0.013	0.011	-0.339
	(0.009)**	(0.008)**	(0.008)*	(0.008)	(0.593)
War (dummy) t-1		-0.506	-0.059	-0.413	0.202
		(0.520)	(0.677)	(0.560)	(0.196)
Unrest (log) t-1			0.232	0.264	-1.964
			(0.169)	(0.169)	(1.125)*
Rel. fractionalization t-1			-1.331	-1.334	-5.399
			(0.665)**	(0.698)*	(13.638)
Mil. capabilities (cube root) t-1				-7.205	-0.008
				(7.759)	(0.014)
NxT	6,164	6,164	6,053	6,052	1,200
Time polynomials	Yes	Yes	Yes	Yes	Yes
Regional dummies	No	No	No	No	No
Mil. regime t-1=1	No	No	No	No	Yes

Multinomial logit regression with cluster-robust standard errors. Columns 1-4 unrestricted sample, base category: non-military regimes. Column 5 restricted sample on military regimes only, base category: indirect military rule. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Robustness tests

We subject our results to a panoply of robustness tests which are all detailed in the Online Appendix. Specifically, we

(i) use the alternative Mundlak-Chamberlain approach to run our fixed effects logit regression (Tables A5-6);

(ii) use a dummy version of regional and center-seeking rebellions instead of the decaying variable (Tables A7-8);

(iii) test a binary indicator of military coups instead of military regimes, receiving very similar findings (Table A9)

(iv) test whether the distance of a regional rebellion to the capital increases the likelihood of military regime emergence, which is an observable implication of our theory (Table A10);

(v) use instrumental variables to take into account the potential endogeneity between military regime and rebellions (Table A11);

(vi) add further or alternative controls, such as ethnic fractionalization, resource rents, population density, trade, a binary control for the post-Cold War period (which by and large has no substantive effect) and a dummy for prior regime type (Tables A12-14);

(vii) control for potential diffusion effects of military regimes and democratization by controlling for the share of military regimes and democracies in the neighborhood, variously defined (Tables A15-17);

(viii) test alternative time specification such as temporal splines (Tables A18-20);

(ix) rerun all our models using a coding of military regimes based on the alternative Geddes et al. (2014) (Tables A21-22);

(x) test alternative half-lives for our decaying variables (2, 3, and 5 years) (Tables A23-A28);

(xi) exclude countries with the greatest leverage on our regression coefficient *Regional rebellion* (Tables A29-20).

As expected, some of these tests render our findings weaker or less precise. That said, most of the robustness tests yield substantively identical or even stronger findings for military regime emergence and militarization. As for potential endogeneity concerns, strong findings from our instrumental variable regression strengthen our claim that it is regional rebellions that cause militarization, not the other way around.

Conclusion

If war makes states, and if states and regimes are empirically intertwined, it stands to reason that different types of wars make different types of regimes. We have found strong quantitative evidence in support of our claim that regional rebellions are systematically more likely than other types of wars to give rise to militarized authoritarian regimes.

This calls new attention to war as a causal variable in the ever-expanding literature on authoritarianism. While the focus in this piece has been on the specific case of *militarized* authoritarian regimes, our findings contribute to the study of authoritarianism more broadly by systematically addressing the question of why different types of authoritarian regimes emerge and endure. In equal measure, our analysis advances the bellicist literature by pivoting the debate from war as an outcome to be explained towards war as a producer of politics. Finally, we refine the literature on civil-military relations and coups by arguing and demonstrating that only one type of armed conflict – regional rebellions – increases the likelihood of military take-overs. We also reemphasize the critical point that "military rule" does not always originate from a coup. Military takeover of power can be gradually negotiated, such as in many Latin

American countries in the 1950s-1970s (e.g. Uruguay), or be relinquished to the military as occurred in the case of Burma in 1958 which is *not* coded as a coup by any of the prominent global coup datasets (Marshall and Marshall 2014; Powell and Thyne 2011).

Much remains to be done to investigate the ways in which rebellions impact regimes. Although the correlation between regional rebellions and political militarization is robust and systematic, it is far more difficult to determine with confidence that the causal mechanisms we posit here are the most important ones driving this result. Future research should both collect further case study evidence and, as far as sources allow, standardized data on the causal mechanisms we have posited to assess their generalizability.

It will also be important to assess why regional rebellions sometimes do *not* give rise to regime militarization. In some cases, fear of a coup might pre-emptively align civilian leaderships with military preferences. Conversely, some categories of cases might be outside of the scope conditions of our theory, as institutional traditions of militaries suppress any political ambitions (as in richer democracies, but probably also some poor democracies like India). Considering how little we know about the effects of different types of war on different types of political regimes, when compared to how much we know about how war makes states, our contribution here is but a modest start on what will hopefully become a much wider and sustained collective scholarly endeavor.

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Online Appendix

Table A1: Summary statistics

	N	Mean	Sd	Min	Max
Military regime	8569	0.170	0.376	0	1
Military regime categorical	8569	0.241	0.560	0	2
Regional rebellion (decay)	16204	0.0442	0.199	0	1
Center-seeking rebellion (decay)	16150	0.0674	0.238	0	1
Previously experienced regional rebellion	16333	0.101	0.302	0	1
Previously experienced center-seeking rebellion	16333	0.226	0.418	0	1
Directly following regional rebellion	3834	0.0764	0.266	0	1
Directly following center-seeking rebellion	3834	0.184	0.388	0	1
Duration of regional rebellion	9619	0.891	5.057	0	66
Duration of ideological rebellion	9619	0.796	3.770	0	50
Regional rebellion (dummy)	9620	0.0681	0.252	0	1
Center-seeking rebellion (dummy)	9620	0.0984	0.298	0	1
GDP p.c. (log)	10416	8.365	1.096	5.7	11.5
Population size (log)	10119	15.22	2.070	9.2	21.0
Polity	8937	0.625	7.488	-10	10
Mil. capabilities (cube root)	8829	126.4	94.61	6.2	726.8
Unrest (log)	9705	0.356	0.676	0	4.5
War (dummy)	14600	0.0170	0.129	0	1
Rel. fractionalization	13277	0.442	0.257	0	0.9
Rents p.c. (log)	9335	2.815	3.013	0	11.4
Population density	12936	226.2	1395.1	0.5	25322.8
Trade	7556	76.46	48.32	0.3	531.7
Post Cold War	9207	0.391	0.488	0	1

Table A2: Countries included in main model

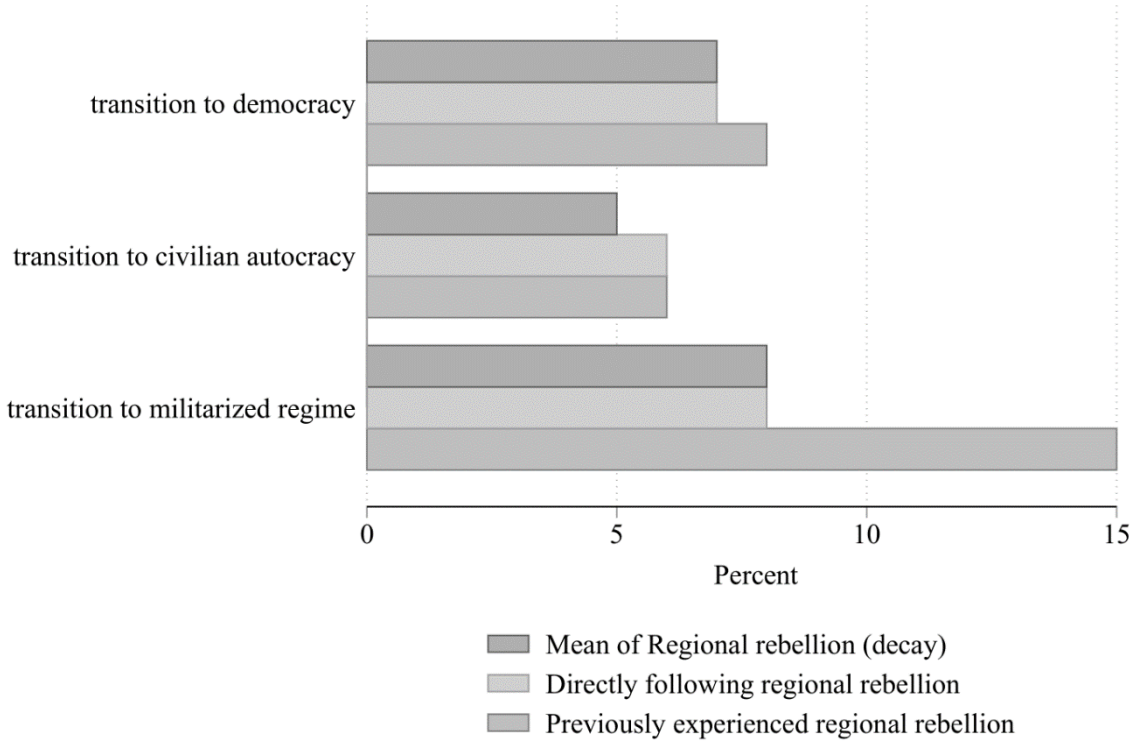
Country	N	Percent	Country	N	Percent
Afghanistan	25	0.53	Latvia	17	0.36
Albania	48	1.02	Lebanon	17	0.36
Algeria	9	0.19	Lesotho	34	0.72
Angola	32	0.68	Liberia	33	0.7
Argentina	33	0.7	Libya	9	0.19
Australia	48	1.02	Lithuania	17	0.36
Austria	48	1.02	Luxembourg	48	1.02
Bahrain	37	0.78	Macedonia	16	0.34
Bangladesh	35	0.74	Madagascar	45	0.95
Belarus	17	0.36	Malawi	44	0.93
Belgium	48	1.02	Malaysia	48	1.02
Benin	23	0.49	Mali	25	0.53
Bhutan	38	0.8	Mauritania	21	0.44
Bolivia	31	0.66	Mauritius	40	0.85
Botswana	42	0.89	Mexico	48	1.02
Brazil	27	0.57	Moldova	17	0.36
Bulgaria	48	1.02	Morocco	48	1.02
Burkina Faso	8	0.17	Mozambique	21	0.44
Burundi	9	0.19	Myanmar	2	0.04
Cambodia	29	0.61	Nepal	43	0.91
Cameroon	48	1.02	Netherlands	48	1.02
Cape Verde	33	0.7	New Zealand	48	1.02
Central African Republic	18	0.38	Nicaragua	29	0.61
Chad	22	0.47	Niger	26	0.55
Chile	31	0.66	Nigeria	11	0.23
China	48	1.02	Norway	48	1.02
Colombia	43	0.91	Oman	48	1.02
Comoros	27	0.57	Pakistan	12	0.25
Congo	13	0.28	Panama	28	0.59
Costa Rica	48	1.02	Papua New Guinea	33	0.7
Croatia	17	0.36	Paraguay	15	0.32
Cuba	48	1.02	Peru	27	0.57
Cyprus	37	0.78	Philippines	46	0.97
Czech Republic	15	0.32	Poland	38	0.8
Democratic Republic of the Congo	7	0.15	Portugal	45	0.95
Denmark	48	1.02	Qatar	37	0.78
Djibouti	31	0.66	Romania	47	0.99
Dominican Republic	43	0.91	Russia	16	0.34
East Timor	6	0.13	Rwanda	12	0.25
Ecuador	36	0.76	Saudi Arabia	48	1.02
Egypt	27	0.57	Senegal	48	1.02
El Salvador	22	0.47	Sierra Leone	34	0.72
Equatorial Guinea	11	0.23	Singapore	43	0.91

Estonia	17	0.36	Slovakia	15	0.32
Ethiopia	15	0.32	Slovenia	17	0.36
Fiji	24	0.51	Solomon Islands	29	0.61
Finland	48	1.02	Somalia	10	0.21
France	48	1.02	South Africa	47	0.99
Gabon	48	1.02	South Korea	20	0.42
Gambia	29	0.61	Spain	31	0.66
Germany	18	0.38	Sri Lanka	33	0.7
Ghana	26	0.55	Sudan	2	0.04
Greece	41	0.87	Suriname	24	0.51
Guatemala	23	0.49	Swaziland	40	0.85
Guinea	24	0.51	Sweden	48	1.02
Guinea-Bissau	18	0.38	Switzerland	48	1.02
Guyana	38	0.8	Syria	12	0.25
Haiti	43	0.91	Tanzania	47	0.99
Honduras	30	0.63	Thailand	30	0.63
Hungary	47	0.99	Togo	9	0.19
India	48	1.02	Trinidad and Tobago	46	0.97
Indonesia	15	0.32	Tunisia	48	1.02
Iran	48	1.02	Turkey	41	0.87
Iraq	23	0.49	Uganda	31	0.66
Ireland	48	1.02	Ukraine	17	0.36
Israel	48	1.02	United Arab Emirates	37	0.78
Italy	48	1.02	United Kingdom	48	1.02
Ivory Coast	39	0.83	Uruguay	36	0.76
Jamaica	47	0.99	Venezuela	48	1.02
Japan	48	1.02	Vietnam	17	0.36
Jordan	48	1.02	Yugoslavia	2	0.04
Kenya	45	0.95	Zambia	44	0.93
Kuwait	40	0.85	Zimbabwe	37	0.78
Laos	36	0.76			

Descriptive patterns for H1 and H2

In addition to presenting the findings from our quantitative models, we explore if our hypotheses are supported by patterns in descriptive data. Figure A1 displays the relative frequency of regional rebellions for three types of regime transitions: transitions toward military regimes, civilian autocracies, and democracies. In the case of transitions to military regimes, the mean of *Regional rebellion (decay)* over the measurement period is 15 and 25 percent higher than for transitions to civilian autocracy and democracy respectively, suggesting that military regimes emerge more often from regional rebellions; second, more transitions to military regimes are directly preceded by a regional rebellion than transitions to other regimes; third, about 15 percent of all transitions to military regimes occur in countries which at some point experienced a regional rebellion, while the equivalent shares in transitions to civilian autocracy and democracy lie around 6 and 8 percent respectively. Taken together, the data suggest that, in line with H1, regime transitions in the wake of regional rebellions are more likely to yield military rule.

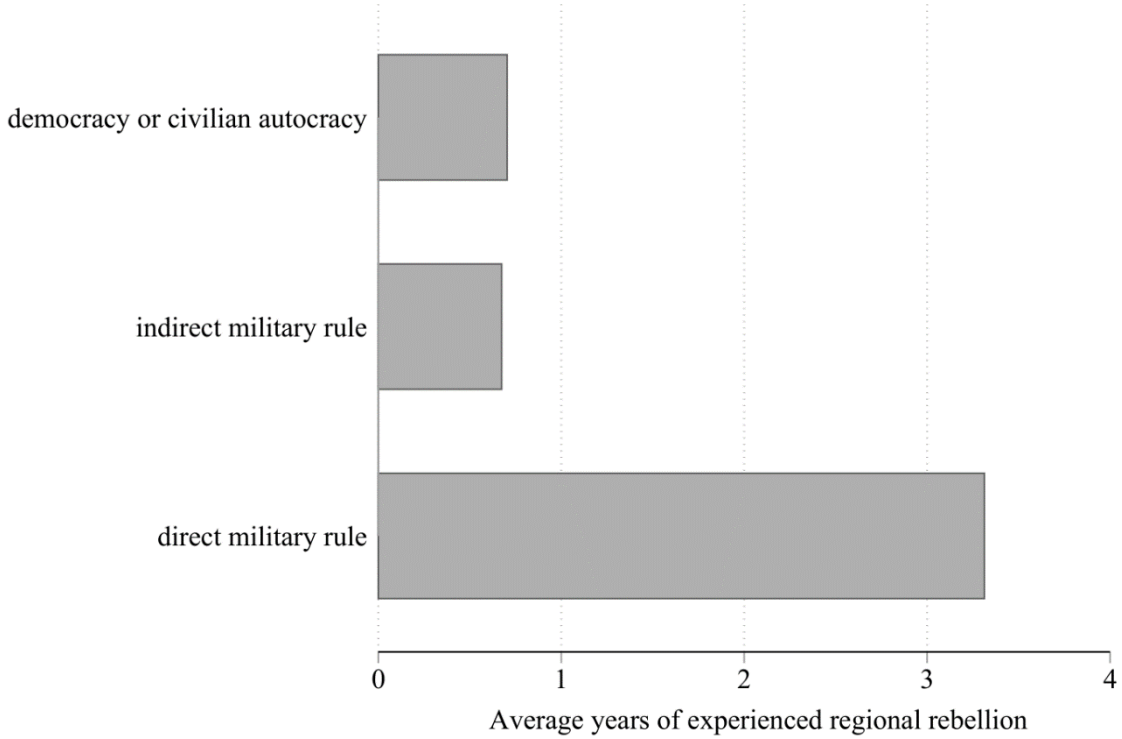
Figure A1: Regional rebellions and regime transitions



*Note: "*Previously experienced regional rebellions" means occurrence of a regional rebellion at some point in the past; "directly following regional rebellions" means a regional rebellion occurred in the previous year. Data on regime types taken from Svolik (2012); rebellion data from Gleditsch et al. (2002).

Figure A2 presents descriptive evidence for our second hypothesis on the intensity of regional conflict and the level of regime militarization. It shows that in regimes under direct military rule the average years of experienced regional rebellions – the mean of our duration measure – is more than three times higher than in democracies, civilian autocracies, or regimes under indirect military rules. This is a first indication that more intense regional conflicts leads to greater militarization of regimes in the form of direct military rule.

Figure A2: Intensity of regional conflict and regime militarization



Note: Data on regime types taken from Svolik (2012); rebellion data from Gleditsch et al. (2002).

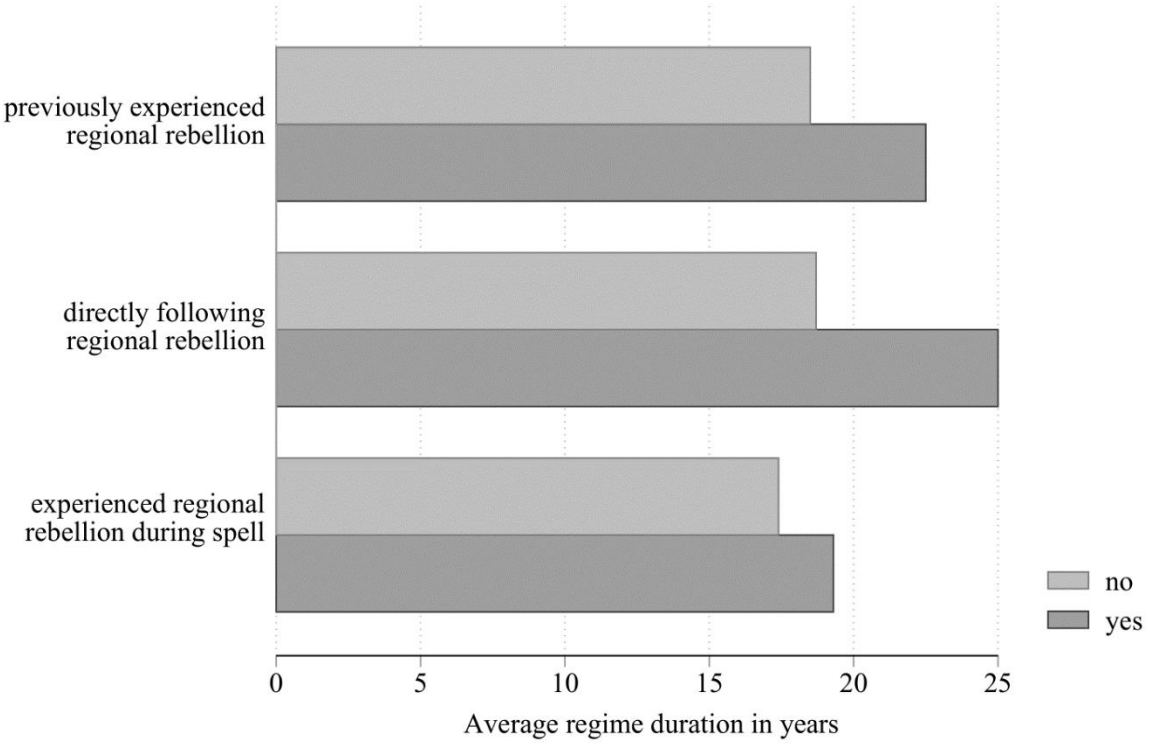
Modelling military regime democratization

While military regimes have, on average, had shorter spells than other regime types (Geddes 1999a), there is actually considerable variation across military regimes, for which our theory provides a potential explanation (Croissant and Kuehn 2016; Kim and Kroeger 2018). Our third hypothesis is thus as follows: Our theoretical argument implies that regional rebellions prompt militaries to hold on to power longer rather than handing it back quickly to civilian elites as their motivation and capacity to do so is stronger. Specifically, we hypothesize the following:

> H3: Military regimes arising in the wake of regional rebellions or exposed to regional rebellions during their spell should withstand pressures for democratization longer than other military regimes.

In terms of descriptive statistics, Figure A3 depicts the average duration of military regimes in years differentiated by whether they experienced regional rebellions. In line with our expectations in H3, the average duration of all military regimes that have experienced some form of regional rebellion is noticeably higher: military regimes that previously experienced a regional rebellion last on average four years longer; regimes that directly emerge in the wake of a regional rebellion last over six years longer; and military regimes facing a regional rebellion during their spell last about two years longer. On the whole, the patterns emerging from the descriptive statistics are in line with our theory and inspire confidence in our theory.

Figure A3: Regional rebellions and military regime duration



Note: "Previously experienced regional rebellions" means occurrence of a regional rebellion at some point in the past; "directly following regional rebellions" means a regional rebellion occurred in the previous year; "experienced regional rebellion during spell" means at least one regional rebellion occurred during the regime spell. Data on regime types taken from Svolik 2012; rebellion data from Gleditsch et al. 2002.

To model the effect of regional rebellions on military regimes' ability to withstand democratization (H3) we use a variant of our Markov transition model presented above. Based on the theory-generating Southeast Asian cases, we hypothesize two scenarios for military regimes to become more resistant to democracy through regional rebellion: first, military regimes that either directly emerge out of a regional rebellion – that is, experience one in the year preceding regime onset – or that were preceded by regional rebellions at some point in the past become more resilient in the face of democratizing forces as the preceding conflict has galvanized military elites into a cohesive ruling group; second, military regimes that experience

regional rebellions during their spell should also be more resistant to democracy as the cohesion of the ruling group is enhanced by conflict. Note that in the latter case military regimes could have come to power for reasons other than regional rebellions but then subsequently become more resilient as a result of regional rebellion.

To capture the foundational effect of preceding regional rebellions, we create two new variables, henceforth called origin variables: *Previously experienced regional rebellion* is a binary variable taking the value of 1 if a military regime was preceded by a regional rebellion at some point before its onset; *Directly following regional rebellion* is also a binary variable indicating whether or not a military regime was preceded by a regional rebellion in the period immediately prior to regime onset. Both variables are time-invariant for the duration of a military regime spell. The effect of rebellions occurring during a spell are captured by our standard decay variables. In terms of the model specification, we follow the setup of the Markov transition model with the exception that we now model the transition from a military regime to democracy. The equation is as follows:

$$\Pr\left(Democracy_{i,t} = 1 \middle| Mil.\ regime_{i,t-1} = 1\right) = Logit(\alpha_0 + \beta_1 R_{i,t-1} + \beta_2 X_{i,t-1} + \delta_3 + \beta_4 \vartheta_{i,t} + \mu_{i,t}), \quad (3)$$

with α designating the constant, R our measures of regional rebellions (origin and during spell), X a vector of covariates, and δ regional fixed effects. The event counter ϑ measures the number of previous regime breakdowns, while μ designates cubic polynomials of the time since military regime onset. As before, we lag all variables except for the event counter and the regional fixed effects,[1] and estimate a pooled as well as a fixed effects logit

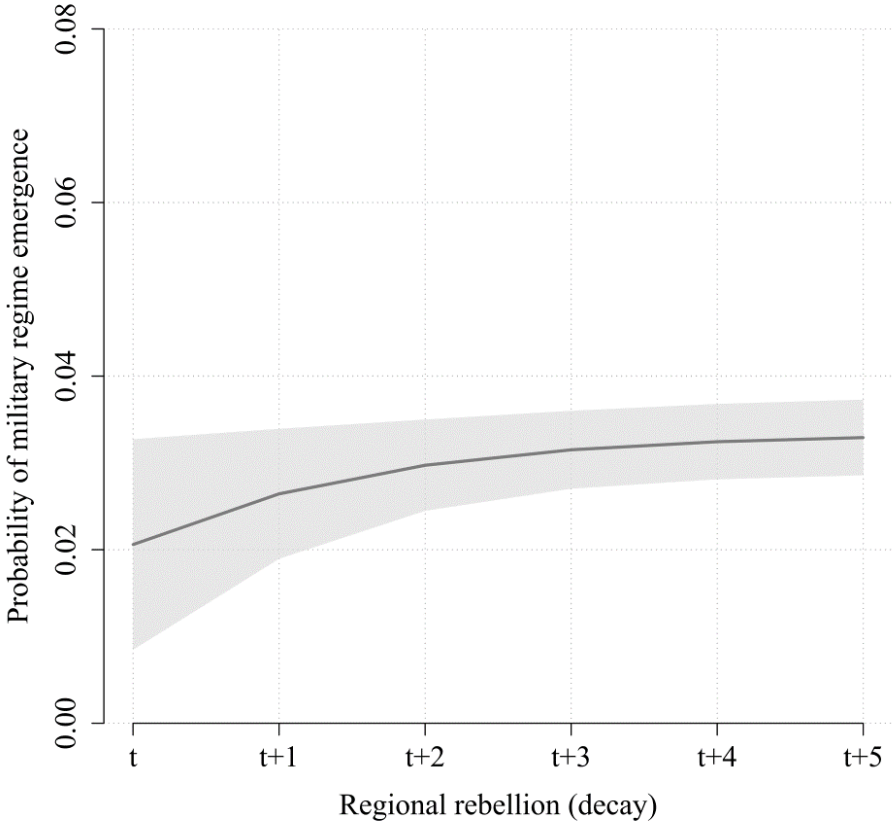
[1] Although our origin variables are time-invariant, we use the one-year lag to bring them in line with the lag structure of the model. Using the contemporaneous version of these variables yields nearly identical results.

model. In the latter, the origin variables are not estimated as they are time-variant so that the model uniquely focuses on the effect of regional rebellions occurring during a military spell.[2]

Our regressions results are displayed in Table A4. Columns 1 and 2 report the pooled logit estimates of the origin variables, while column 3 shows the estimates for rebellions occurring during a regime spell only. Columns 4 and 5 show the results when both measures of rebellion are added simultaneously into the regression. The conditional logit results are shown in Column 6. On the whole, the findings partially support H3. On the one hand, rebellions occurring during regime spells significantly reduce the likelihood of a transition to democracy. Given a baseline probability of democratization of 3.3 percent in the pooled model, the occurrence of a regional rebellions reduces this probability on average by 1.5 percent absolutely and 45 percent relatively speaking. The effect then fades out over the five following periods (see Figure A4). The conditional logit model which focuses solely on within regime variation yields a similarly, slightly stronger finding as significance levels increase.

[2] We also tested another variant of the Markov model where we keep *Military regime* as our dependent variable and condition on *Military regime*$_{t-1}$ =1. While this model slightly shifts the focus from democratization to military regime breakdown more generally, the main finding is the same in that regional rebellions during regime spells increase the durability of military regimes. Results are available upon request.

Figure A4: Effect of regional rebellion on military regime democratization



Note: Marginal effects derived from Table A4, column 5. *Regional rebellion* set at 1, 0.5, 0.25, 0.125, and 0.0625 according to decay function. All other covariates set at their observed values (Hanmer and Ozan Kalkan 2013).

On the other hand, our expectation that regimes emerging out of, or in contexts preceded by regional rebellions are more stable, does not bear out. In fact, all signs are positive except in column 1. The results of column 5 suggest that regimes directly following a regional rebellion are more likely to break down than regimes without previous rebellions. This finding is somewhat surprising in light of the suggestive descriptive statistics presented at the outset of this section. We see two possible reasons for this: First, with a total of three cases (Indonesia, Mauritania, Myanmar), military regimes arising directly in the wake of regional rebellion are relatively rare in our data, and while their spell is comparatively long (32, 26, and 47 years

respectively), the model might struggle to produce clear results from so few positive observations. Second, a number of regimes are double-coded as having both regional and center-seeking rebellions prior to their onset (see Table A3). This is, amongst others, the case for Myanmar – one of the most long-lasting military dictatorships. As a result, differentiating the effect of center-seeking and regional rebellions at the origin of military regimes is complicated because of tied data.[3] Conversely, given that these tied data bias the results against our hypotheses more generally, the fact that in all our models *Regional decay* has been highly significant makes us more confident in the validity of our findings.

Table A3: Regimes with both types of rebellion

Previously experienced regional AND center-seeking rebellion	***Directly following regional AND center-seeking rebellion***
Comoros (3)	Myanmar (46)
D. R. Congo (26, 3)	
Ethiopia (15, 2)	
Indonesia (32)	
Myanmar (45)	
Niger (1)	
Nigeria (12, 14)	
Pakistan (8)	

Note: Duration of military regime spell in brackets.

[3] In additional tests, we experimented with recoding all tied origin variables as regional rebellions only. This removes the positive significant effect of the regional origin variable but still does not show a negative significant effect on democratization. Results are available upon request.

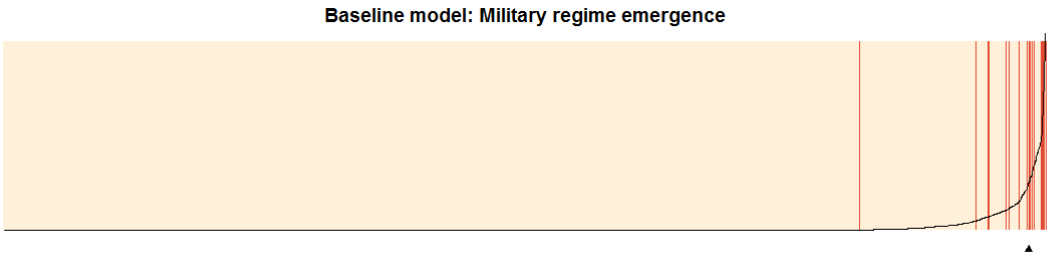
Table A4: Regional rebellions and military regime democratization

	(1)	(2)	(3)	(4)	(5)	(6)
Previously experienced regional rebellion t-1	0.759			2.161		
	(1.190)			(1.816)		
Previously experienced center-seeking rebellion t-1	0.844			-0.018		
	(0.861)			(1.378)		
Directly following regional rebellion t-1		0.650			2.212	
		(0.879)			(0.965)**	
Directly following center-seeking rebellion t-1		0.029			-0.775	
		(1.483)			(1.343)	
Regional rebellion (decay) t-1			-2.613	-3.884	-1.808	-24.995
			(1.496)*	(1.858)**	(0.975)*	(6.222)***
Center-seeking rebellion (decay) t-1			1.713	1.930	1.625	-1.636
			(1.006)*	(1.513)	(1.131)	(2.287)
GDP p.c. (log) t-1	0.088	0.497	0.471	0.165	0.558	-1.891
	(0.807)	(1.146)	(0.973)	(0.765)	(0.964)	(8.406)
Population size (log) t-1	-1.501	-0.628	-0.881	-1.361	-0.382	9.899
	(1.380)	(1.180)	(1.247)	(1.211)	(1.002)	(6.658)
Polity t-1	0.814	0.786	0.829	0.854	0.802	1.528
	(0.099)***	(0.110)***	(0.119)***	(0.134)***	(0.103)***	(0.678)**
Mil. capabilities (cube root) t-1	34.619	14.964	22.660	30.100	9.588	365.337
	(29.913)	(23.793)	(26.368)	(25.568)	(21.430)	(347.806)
Unrest (log) t-1	-0.184	-0.684	-0.187	-0.139	-0.678	-3.729
	(0.595)	(0.661)	(0.586)	(0.552)	(0.554)	(3.063)
War (dummy) t-1	-1.963	-2.434	-2.892	-2.906	-3.342	-3.161
	(1.387)	(1.410)*	(1.486)*	(1.578)*	(1.470)**	(2.580)
Rel. fractionalization t-1	4.988	5.747	5.961	5.741	6.710	
	(2.925)*	(2.132)***	(2.871)**	(2.865)**	(2.426)***	
Number of previous regime failures	0.697	0.805	0.671	0.767	0.911	-2.812
	(0.385)*	(0.419)*	(0.328)**	(0.459)*	(0.370)**	(1.452)*
N	1,166	1,023	1,166	1,166	1,023	471
Time polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes

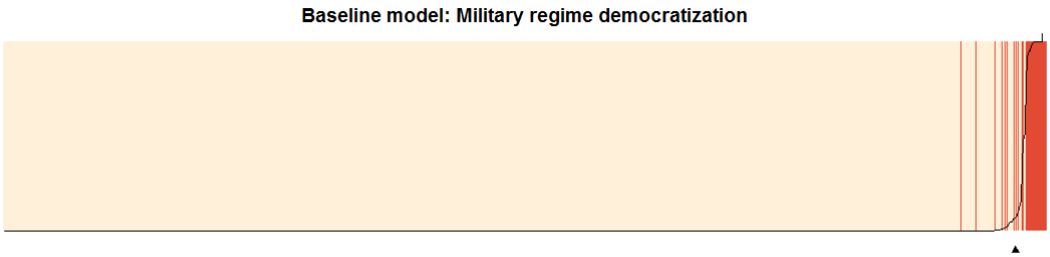
Pooled logit model in columns 1-5. Conditional logit model in column 6. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0$.

Separation plots to assess model fit

The two below figures show separation plots for our two baseline models. Separation plots arrange the fitted values in ascending order of probability with each line representing one observation (Greenhill, Ward, and Sacks 2011). Red lines are observations in which the outcome of interest actually occurs. The added line represents the predicted probabilities estimated for each observation. The small triangle beneath the plot indicates the expected number of total events predicted. A perfectly fitted model would thus have red and yellow lines perfectly separated at the position of the triangle, with a sharp corresponding rise in the predicted probability line. A very bad model would see the occurrence of actual outcomes at even very low levels of predicted probability and a slow gradual increase of the probability line. Bearing this in mind, the graphs below demonstrate a good model fit with high levels of separation and a visible spike in the predicted probability at the triangle separation point.



Note: Figure based on column 4, Table 1 in the main article.



Note: Figure based on column 3, Table A4 in the Online Appendix.

General note on robustness tests

The below section includes the following tests: additional/alternative variables, diffusion effects, and alternative time specifications. The tests are carried out for our military regime emergence and democratization models. Wherever possible, these tests are also carried out for our multinomial logit model of regime militarization.

Mundlak-Chamberlain models

As an alternative to our conditional logistic fixed effects regressions, we estimate the effect of rebellions on the emergence and democratization of military regimes using the Mundlak-Chamberlain (Chamberlain 1982; Mundlak 1978) approach. The model takes the following functional form:

$$\Pr\left(Y_{i,t} = 1 \middle| Y_{i,t-1} = 0\right) = Logit\left(\alpha_0 + \beta_1\left(R_{i,t-1} - \bar{R}_\iota\right) + \beta_2\left(X_{i,t-1} - \bar{X}_\iota\right) + \beta_3\bar{R}_\iota + \beta_4\bar{X}_\iota + +\beta_5\bar{Y}_\iota + \delta_3 + \mu_{i,t}\right), \quad (1),$$

where α designates the constant, *R* our measure of regional rebellions, *X* a vector of covariates (including our event count variable), and δ regional fixed effects. By adding the country means and the yearly deviation from the country means, the model enables us to separate cross-country from within-country effects. Note that in Table A6 we refrain from adding the yearly deviation for the variables measuring the rebellion prior or at regime onset (*Previously experienced regional/center-seeking rebellion*; *directly following regional/center-seeking rebellion*) as these are highly collinear with our standard measure of *Regional/Center-seeking rebellion*. Additionally, we add the country mean of our dependent variable *Y*, which allows us to account for unit heterogeneity by, effectively, adding a varying intercept (Wright, Frantz, and Geddes 2015). We also add μ, a vector of cubic time polynomials to

account for duration dependence (Carter and Signorino 2010). As before, all variables except for regional fixed effects and the event counter are lagged by one year to mitigate endogeneity concerns and ensure the correct order of causal dependence.

The results for military regime emergence are shown in Table A5. In line with our baseline model, the model finds that within-country changes of regional rebellions significantly increase the likelihood of military regime emergence. The effect is illustrated in the marginal effects plot in Figure A5. By contrast, cross-country differences in the average level of regional rebellions are not associated with a higher likelihood of military regime emergence, suggesting that our finding is driven by within-country variation of regional rebellions over time.

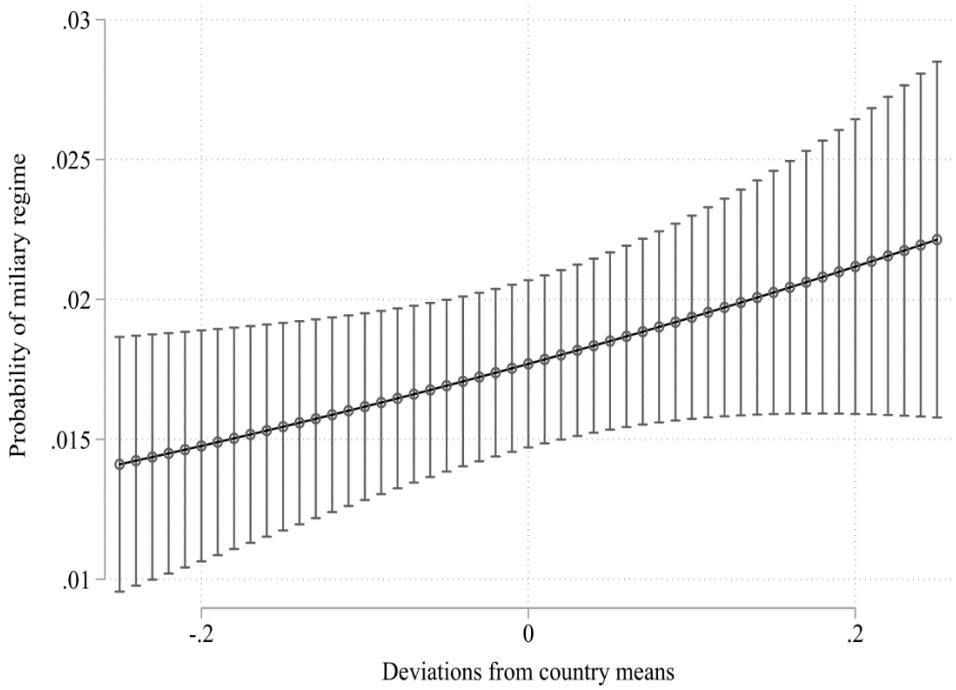
The results for military regime democratization are shown in Table A6. None of the variables for regional rebellions is significant. As we explain in the article, this is most probably due to tied data for regimes that experience both a regional and a center-seeking rebellion prior to regime onset.

Table A5: Mundlak-Chamberlain estimation of military regime emergence

	(1)	(2)
Regional rebellion (decay, mean)	-0.558	-0.682
	(0.979)	(1.569)
Regional rebellion t-1 (decay, dev)	1.607	1.308
	(0.762)**	(0.755)*
Center-seeking rebellion (decay, mean)	1.476	0.496
	(0.671)**	(0.649)
Center-seeking rebellion t-1 (decay, dev)	0.391	0.077
	(0.540)	(0.607)
GDP p.c. (log, mean)	-0.042	-0.218
	(0.286)	(0.261)
GDP p.c. t-1 (log, dev)	-0.036	-0.071
	(0.533)	(0.553)
Population size (log, mean)	0.201	0.292
	(0.292)	(0.300)
Population size t-1 (log, dev)	2.661	3.659
	(0.849)***	(0.876)***
Polity (mean)	0.043	0.162
	(0.050)	(0.078)**
Polity t-1 (dev)	-0.265	-0.365
	(0.050)***	(0.064)***
Mil. capabilities (cube root, mean)	-7.251	-5.096
	(8.429)	(7.096)
Mil. capabilities t-1 (cube root, dev)	-16.044	-18.931
	(18.689)	(19.885)
Unrest (log, mean)	0.881	1.008
	(0.769)	(0.671)
Unrest t-1 (log, dev)	0.215	-0.012
	(0.203)	(0.226)
War (dummy, mean)	4.432	3.892
	(3.496)	(2.916)
War t-1 (dummy, dev)	0.746	-0.027
	(0.741)	(0.886)
Rel. fractionalization (mean)	-0.793	-0.211
	(0.841)	(0.725)
Previous failures (mean)	0.147	-0.011
	(0.029)***	(0.043)
Previous failures (dev)	-0.191	-0.128
	(0.031)***	(0.022)***
Y (mean)		8.353
		(1.720)***
NxT	4,630	4,630
Time polynomials	Yes	Yes
Regional dummies	Yes	Yes

Mundlak-Chamberlain logistic regression with cluster-robust standard errors in parentheses. *Mean* designates the country averages, *dev* designates the annual deviation from country averages. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Figure A5: Effect of within-country changes in regional rebellions



Note: Marginal effects derived from Table A5, column 1. All other covariates set at their observed values (Hanmer and Ozan Kalkan 2013).

Table A6: Mundlak-Chamberlain estimation of military regime democratization

	(1)	(2)	(3)	(4)	(5)
Previously experienced regional rebellion (mean)	2.777			10.831	
	(3.403)			(9.747)	
Previously experienced center-seeking rebellion (mean)	-1.022			-5.283	
	(1.256)			(3.448)	
Directly following regional rebellion (mean)		0.901			-18.083
		(4.173)			(32.346)
Directly following center-seeking rebellion (mean)		-1.847			-3.842
		(2.227)			(5.472)
Regional rebellion (decay, mean)			-4.167	-18.157	8.392
			(8.185)	(14.272)	(21.409)
Regional rebellion t-1 (decay, dev)			-30.648	-6.200	-71.368
			(43.700)	(13.656)	(54.773)
Center-seeking rebellion (decay, mean)			11.036	20.118	19.620
			(7.938)	(10.891)*	(9.216)**
Center-seeking rebellion t-1 (decay, dev)			2.886	3.987	2.456
			(1.757)	(2.431)	(1.736)
Y (mean)	44.420	51.410	-30.814	-41.172	-93.795
	(40.531)	(47.585)	(38.599)	(48.098)	(64.942)
NxT	1,166	1,151	1,166	1,166	1,151
Standard controls	Yes	Yes	Yes	Yes	Yes
Time polynomials	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes

Mundlak-Chamberlain logistic regression with cluster-robust standard errors in parentheses. *Mean* designates the country averages, *dev* designates the annual deviation from country averages. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Dummy instead of decaying variables for rebellion

To ascertain that our findings are not purely an artefact of our decaying measure of rebellions, we re-run our baseline models for military regime emergence and democratization using a simple dummy indicator of rebellion. The variable measures if a regional or center-seeking rebellion occurred in the previous year. The results are shown in Table A7 and Table A8.

As expected, constraining the effect of rebellions to one period weakens the association between regional rebellions and military regime emergence and democratization. That said, the pattern we have seen when using decaying variables reemerges when using simple dummies: In Table A7, only columns 4 and 5 are slightly insignificant, but with p=0.105 and 0.125 respectively reasonably close given that we are using a much coarser measure of regional rebellions. And in Table A8, we find a similar pattern to our baseline democratization model, suggesting that regional rebellions occurring during a military regime spell significantly decrease the likelihood of democratization. This is particularly the case when focusing on within-country variation only (Table A8, column 6), which is, as we know from Table A5, the main driver of our finding.

Table A7: Regional rebellions (dummy) and military regime emergence

	(1)	(2)	(3)	(4)	(5)
Regional rebellion (dummy) t-1	0.849	0.859	0.750	0.709	1.421
	(0.457)*	(0.455)*	(0.455)*	(0.438)	(0.920)
Center-seeking rebellion (dummy) t-1	0.422	0.422	0.320	0.281	0.918
	(0.336)	(0.336)	(0.317)	(0.324)	(0.610)
GDP p.c. (log) t-1	-0.166	-0.167	-0.179	0.001	-0.952
	(0.240)	(0.239)	(0.235)	(0.267)	(0.852)
Population size (log) t-1	0.057	0.058	0.006	0.379	2.455
	(0.126)	(0.123)	(0.122)	(0.213)*	(2.106)
Polity t-1	-0.125	-0.126	-0.140	-0.152	-0.408
	(0.031)***	(0.031)***	(0.032)***	(0.035)***	(0.098)***
Previous failures	0.181	0.182	0.156	0.114	-0.926
	(0.211)	(0.212)	(0.235)	(0.249)	(0.945)
War (dummy) t-1		-0.114	0.054	0.385	0.383
		(0.766)	(0.746)	(0.734)	(1.053)
Unrest (log) t-1			0.483	0.519	-0.102
			(0.150)***	(0.153)***	(0.306)
Rel. fractionalization t-1			-1.562	-1.678	
			(0.692)**	(0.746)**	
Mil. capabilities (cube root) t-1				-10.067	-12.692
				(5.041)**	(32.218)
NxT	4,704	4,704	4,613	4,612	1,301
Time polynomials	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-4. Conditional logit model in column 5. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A8: Regional rebellions (dummy) and military regime democratization

	(1)	(2)	(3)	(4)	(5)	(6)
Previously experienced regional rebellion t-1	0.759			1.575		
	(1.190)			(1.421)		
Previously experienced center-seeking rebellion t-1	0.844			0.611		
	(0.861)			(1.053)		
Directly following regional rebellion t-1		0.650			1.686	
		(0.879)			(0.983)*	
Directly following center-seeking rebellion t-1		0.029			-0.374	
		(1.483)			(1.582)	
Regional rebellion (dummy) t-1			-2.286	-3.422	-1.233	-16.375
			(1.491)	(1.765)*	(1.023)	(2.434)***
Center-seeking rebellion (dummy) t-1			1.160	1.047	0.822	-3.039
			(1.116)	(1.364)	(1.338)	(2.106)
GDP p.c. (log) t-1	0.088	0.497	0.388	0.209	0.490	-4.731
	(0.807)	(1.146)	(0.942)	(0.754)	(1.034)	(7.567)
Population size (log) t-1	-1.501	-0.628	-0.949	-1.368	-0.523	7.758
	(1.380)	(1.180)	(1.215)	(1.268)	(1.032)	(10.625)
Polity t-1	0.814	0.786	0.815	0.821	0.790	1.989
	(0.099)***	(0.110)***	(0.109)***	(0.105)***	(0.101)***	(0.750)***
Mil. capabilities (cube root) t-1	34.619	14.964	23.762	30.289	12.450	481.046
	(29.913)	(23.793)	(25.888)	(27.057)	(21.284)	(291.278)*
Unrest (log) t-1	-0.184	-0.684	-0.170	-0.131	-0.657	-4.846
	(0.595)	(0.661)	(0.599)	(0.584)	(0.607)	(2.408)**
War (dummy) t-1	-1.963	-2.434	-2.583	-2.744	-2.903	-4.279
	(1.387)	(1.410)*	(1.567)*	(1.582)*	(1.568)*	(2.346)*
Rel. fractionalization t-1	4.988	5.747	5.707	5.148	6.195	
	(2.925)*	(2.132)***	(2.746)**	(2.855)*	(2.242)***	
Number of previous regime failures	0.697	0.805	0.638	0.763	0.852	-2.662
	(0.385)*	(0.419)*	(0.318)**	(0.367)**	(0.412)**	(1.451)*
NxT	1,166	1,023	1,166	1,166	1,023	471

Time polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-5. Conditional logit model in column 6. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Using coups as dependent variable

As we argue in the paper, the concept of militarization overlaps with but is not identical to military coups. To test the degree of overlap empirically, we rerun our baseline logit model using a binary indicator for coups instead of militarized regimes as our dependent variable. We test coup data from the Powell and Thyne (2011) (column 1) and Marshall and Marshall (2014) (column 2). We would expect the direction of the sign and strength of the sign to be similar. This is precisely what Table A9 shows. Like in our baseline model, the coefficient of *Regional rebellion* is positive; the association is slightly weaker when using the Powell and Thyne dataset with p=0.15.

Table A9: Regional rebellions and military coups

	(1)	(2)
Regional rebellion (decay) t-1	0.384	0.514
	(0.264)	(0.260)**
Center-seeking rebellion (decay) t-1	0.238	0.297
	(0.153)	(0.138)**
GDP p.c. (log) t-1	-0.358	-0.397
	(0.100)***	(0.090)***
Population size (log) t-1	0.006	-0.069
	(0.109)	(0.098)
Polity t-1	-0.007	-0.012
	(0.012)	(0.011)
War (dummy) t-1	-0.099	0.169
	(0.410)	(0.332)
Unrest (log) t-1	0.303	0.336
	(0.106)***	(0.093)***
Rel. fractionalization t-1	-0.278	-0.071
	(0.299)	(0.288)
Mil. capabilities (cube root) t-1	-1.573	-2.121
	(2.660)	(2.621)
Number of previous coups	0.002	0.014
	(0.019)	(0.021)
NxT	6,263	6,260
Time polynomials	Yes	Yes
Regional dummies	Yes	Yes

Pooled logit model with cluster-robust standard errors in parentheses. Column 1 uses the Powell and Thyne data; column 2 uses the Marshall and Marshall data * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Testing the effect of rebellion distance to capital

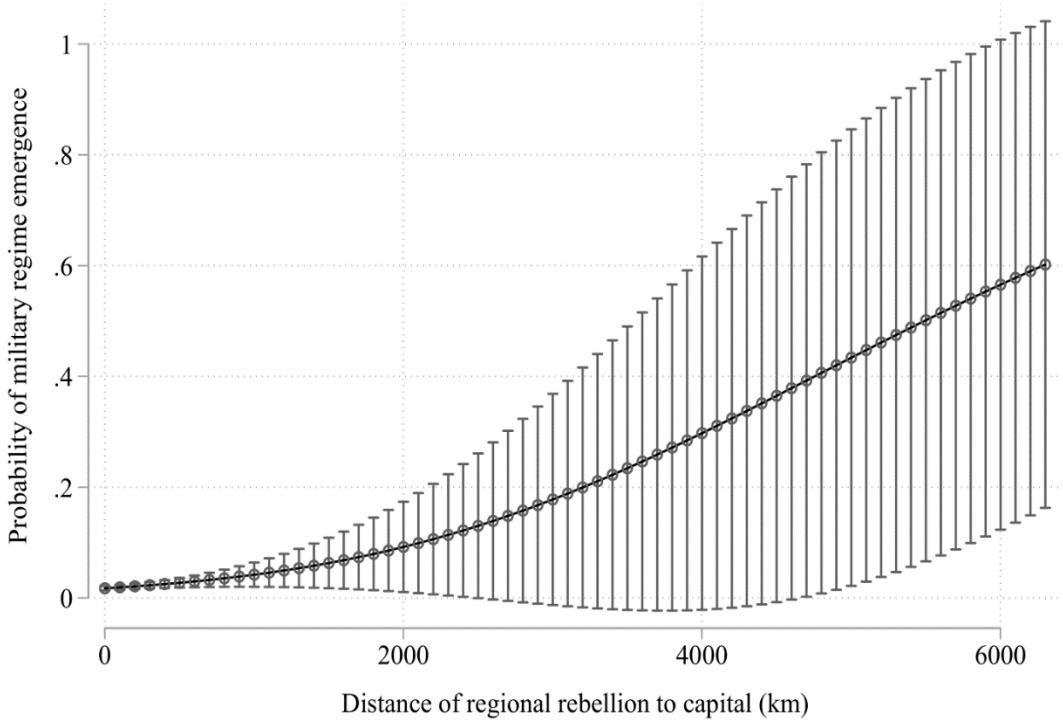
The proposed disalignment mechanism implies strong information asymmetries between civilian and military elites as a result of the distance between the regional rebellion and urban centers, especially the capital. An observable implication of this mechanism is that the effect of regional rebellions on the likelihood of military regime emergence should increase the farther the rebellion is away from the capital. We test this corollary using the geocoded version of the PRIO conflict dataset (1946-2005; Allansson, Melander, and Themnér 2017; Gleditsch et al. 2002). The new distance-weighted variables measure the distance of a rebellion from the capital city, with 0 indicating no rebellion, 1 a rebellion in the capital city, and further integers signifying larger distances from the capital. The results are shown in Table A10 below and strongly confirm our prior. The coefficient of *Regional rebellion* is strongly positively significant, meaning the farther a rebel a regional rebellion is away from the capital, the more likely the emergence of a military regime becomes. This effect is illustrated graphically in below. Interestingly, while the effect is only weakly significant, even center-seeking rebellion begin to exhibit a similar pattern once their distance to the capital city is taken into account and, as our theory predicts, information asymmetries become more salient.

Table A10: Rebellion distance and military regime emergence

	(1)
Regional rebellion (distance-weighted) t-1	0.001
	(0.000)***
Center-seeking rebellion (distance-weighted) t-1	0.004
	(0.002)*
GDP p.c. (log) t-1	-0.009
	(0.280)
Population size (log) t-1	0.402
	(0.211)*
Polity t-1	-0.160
	(0.038)***
War (dummy) t-1	0.042
	(0.733)
Unrest (log) t-1	0.488
	(0.167)***
Rel. fractionalization t-1	-1.756
	(0.786)**
Mil. capabilities (cube root) t-1	-10.661
	(5.053)**
Previous failures	0.144
	(0.234)
NxT	4,612
Time polynomials	Yes
Regional dummies	Yes

Pooled logit model with cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Figure A6: Capital distance of rebellions and militarized regimes



Note: Marginal effects derived from Table A10, column 1. All other covariates set at their observed values (Hanmer and Ozan Kalkan 2013).

Instrumental variable approach

A potential source of bias is endogeneity and reversed causality. Military repression, for example, might aggravate regional grievances, thus leading to regional rebellions rather than vice-versa. Indeed, Fjelde (2010) finds that military regimes are more likely to become involved in civil wars. Moreover, both regional rebellions and military regimes might be jointly associated with an uncontrolled omitted confounder. Depending on the strength of endogeneity and/or reverse causality, both scenarios could considerably bias our findings. The standard econometric approach to this problem has been to use instrumental variables. Finding instruments that are both sufficiently predictive and only influence the outcome *Y* through its effect on *X* (exclusion restriction) is often challenging, however (Bollen 2012; Sovey and Green 2011). In our case, this challenge is aggravated by the fact that we need to instrument for two endogenous predictors as both regional and center-seeking rebellions can plausibly be considered endogenous and/or in a reverse-causal relationship with military regimes.

We believe to have identified two instruments that fulfil the stringent conditions for instrumental variables: First, taking into account the emphasis the civil war literature has placed on the geographical conditions conducive to civil wars (e.g. Fearon and Laitin 2003), we turn to terrain characteristics as an instrument. Both center-seeking and separatist rebels will have to hide at times from government forces, which can be greatly facilitated or complicated by the shape of the terrain. The varying availability of hideouts is thus likely to play some role in the decision of whether or not to mount a rebellion. We therefore use *Rugged terrain in (thousands of) ha (log) t-1* taken from Nunn and Puga (2012) as our first instrument. Second, building on the insights from diffusion arguments of macro-social outcomes (Gleditsch and Ward 2006; Miller 2015), we use the number of *Battle-related deaths in neighboring countries t-1* as an instrument for regional and center-seeking

rebellion. Our logic relies on an implicit deterrence mechanism from large-scale human losses in neighboring countries on potential rebels. Neighborhood is defined as all contiguous states not separated by more than 24 miles of water.[4] We use the Correlates of War Direct Contiguity dataset (Stinnett et al. 2002, v3.2) to construct these variables.

Could it be that that terrain characteristics and battle-related deaths in the neighborhood have a direct effect on military regime emergence or breakdown that is not mediated by domestic rebellion? Existing theory does not suggest this. Geography can plausibly be considered as exogenous. As for the second instrument, violence in neighboring countries could certainly increase the readiness of domestic military units in order to prevent spill-over effects; yet the mechanisms that would lead from, say, increased border patrols to a military takeover seem to be rather obscure.

Technically, we conduct our instrumental variable regression using a limited-information maximum likelihood estimator (LIML) as such estimators have been shown to be more robust to weak instrumental variable bias (Stock and Yogo 2005).[5] Fortunately, all our F statistics meet the Stock-Yogo (2005) critical value on instrument strength. This suggests a rather strong predictive power of our instruments, so that any potential weak instrument bias will be limited. The results of the IV regressions are shown in Table A11. In line with our baseline model, regional rebellions remain a strong predictor for the emergence of military regimes. We do not recover, however, our previous finding regarding military regime democratization: while the sign of regional rebellion remains negative, the coefficient is far from statistical significance. This emphasizes the merely indicative nature of our finding on democratization at this stage.

[4] This is one of the possible operationalizations of contiguity proposed in the dataset. Results are similar for alternative definitions of contiguity and are available upon request.

[5] We use the *ivreg2* command in Stata to run these regressions.

Table A11: Regional conflict and military regime emergence/democratization (IV approach)

	(1)	(2)
Second stage outcome	Military regime *emergence*	Military regime *democratization*
Regional rebellion (decay) t-1	0.232**	-0.0434
	(0.112)	(0.180)
Center-seeking rebellion (decay) t-1	-0.201	0.0274
	(0.151)	(0.0787)
First stage outcome	Regional rebellion decay t-1	Regional rebellion decay t-1
Battle-related deaths in neighboring countries t-1	-0.004***	-0.0084**
	(0.0015)	(0.004)
Rugged terrain in ha (log) t-1	0.0054	0.0085
	(0.0033)	(0.0069)
First stage outcome	Center-seeking rebellion decay t-1	Center-seeking rebellion decay t-1
Battle-related deaths in neighboring countries t-1	0.0015	0.0020
	(0.0022)	(0.0058)
Rugged terrain in ha (log) t-1	0.0065**	0.0441***
	(0.0026)	(0.0159)
N x T second stage	4,889	1,093
Time polynomials	Yes	Yes
Regional dummies	Yes	Yes
Standard controls	Yes	Yes
Cragg-Donald Wald F statistic	7.73	11.62
Stock-Yogo critical value	7.03	7.03

Two-stage instrumental variable regression with limited-information maximum likelihood estimator (LIML) and cluster-robust standard errors in parentheses. The Cragg-Donald Wald F statistic and the Stock-Yogo critical values are taken from a model assuming conditional homoscedasticity as the Stock-Yogo test is only valid in the presence of i.i.d. errors. * p<0.1; ** p<0.05; *** p<0.01

Additional control variables

As it is standard practice, we test the robustness of our baseline models to the addition of further control variables. Specifically, we add *Oil rents (log)* (Ross 2013), *Population density* (UNDP 2015), *Trade/GDP* (World Bank 2017), a dummy for the post-Cold War period (*Post Cold War*), and a measure of the prior regime type (*Prior regime*) (Svolik 2012) to the regression. The latter is a categorical variable measuring if a regime is autocratic, democratic, or there was no authority. These additional variables aim to take into account that (i) military regimes might be more likely to emerge in resource-abundant countries; (ii) civilian involvement in counter-insurgency might be more likely in more densely populated areas, decreasing the likelihood of military regime emergence; (iii) more open economies might exhibit more volatile business cycles, ensuing instability and potentially military take-overs or regime breakdowns; (iv) military regimes might be less likely to emerge in the post-Cold War era; and (v) certain regime types might be more likely to transition into military rule. We run all these regressions using *Ethnic fractionalization* instead of religious fractionalization as control variable to make sure our findings are robust to ethnic diversity (Alesina et al. 2003).

In the light of the results displayed in Table A12-Table A14, our main findings appear practically unchanged by these additional variables. Regional rebellions remain a highly significant predictor of military take-overs; and regional rebellions occurring during a regime spell tend to decrease the likelihood of military regime democratization. The latter finding seems to be particularly driven by within-country variation (see the conditional logit model in Table A14, column 6). As before, our expectation that regimes emerging out of or in contexts of regional rebellions is not supported by the model, as shown by the positive significant effect for *Previously experienced regional rebellion* and *Directly following regional rebellion*. As we discussed in the article, while we are puzzled by these findings in

the light of strong descriptive patterns, the absence of a clear finding might have to do with the effect of ties in our data.

As for the multinomial logit model on regime militarization (Table A13), the effect of rebellion intensity remains highly significant in the unrestricted sample (column 1). In the restricted sample (column 2) for military regimes only, the coefficient of *Duration of regional rebellion* becomes slightly insignificant (p=0.155). At the same, we lose about 150 observations in this regression compared to our baseline model, which is likely to be driving the loss of significance.

Table A12: Regional rebellions and military regime emergence (additional controls)

	(1)	(2)	(3)	(4)	(5)	(6)
Regional rebellion (decay) t-1	0.923	0.950	1.159	1.129	0.913	2.041
	(0.420)**	(0.416)**	(0.478)**	(0.488)**	(0.536)*	(1.022)**
Center-seeking rebellion (decay) t-1	0.171	0.200	0.256	0.293	0.147	1.366
	(0.353)	(0.354)	(0.379)	(0.380)	(0.428)	(0.943)
Ethnic fractionalization t-1	1.175	0.736	0.665	0.698	1.099	
	(0.738)	(0.742)	(0.772)	(0.773)	(0.732)	
Oil rents (log) t-1	-0.082	-0.088	-0.043	-0.041	-0.064	0.372
	(0.061)	(0.062)	(0.071)	(0.070)	(0.068)	(0.396)
Pop. density t-1		-0.003	-0.003	-0.003	-0.002	-0.039
		(0.002)	(0.002)	(0.002)	(0.002)	(0.053)
Trade/GDP t-1			-0.009	-0.007	-0.006	0.010
			(0.008)	(0.008)	(0.008)	(0.013)
Post Cold War				-0.408	-0.524	-0.540
				(0.355)	(0.365)	(1.062)
Prior regime: Autocracy t-1					-1.434	0.256
(*baseline cat.: democracy*)					(0.713)**	(0.953)
Prior regime: No authority t-1					-0.819	-0.516
(*baseline cat.: democracy*)					(0.997)	(1.615)
NxT	4,605	4,605	4,092	4,092	4,092	1,099
Time polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes
Standard controls	Yes	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-5. Conditional logit model in column 6. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A13: Regional rebellions and regime militarization (additional controls)

		(1)	(2)
Indirect Military	Duration of regional rebellion t-1	0.041	
		(0.043)	
	Duration of center-seeking rebellion t-1	0.018	
		(0.029)	
	Ethnic fractionalization t-1	-0.027	
		(0.632)	
	Oil rents (log) t-1	-0.003	
		(0.057)	
	Pop. density t-1	-0.000	
		(0.001)	
	Trade/GDP t-1	0.003	
		(0.004)	
	Post Cold War	0.191	
		(0.269)	
	Prior regime: Autocracy t-1	0.165	
	(*baseline cat.: democracy*)	(0.355)	
	Prior regime: No authority t-1	-1.234	
	(*baseline cat.: democracy*)	(0.588)**	
Direct Military	Duration of regional rebellion t-1	0.117	0.073
		(0.028)***	(0.051)
	Duration of center-seeking rebellion t-1	-0.108	-0.125
		(0.034)***	(0.049)**
	Ethnic fractionalization t-1	-0.430	-0.324
		(1.029)	(1.303)
	Oil rents (log) t-1	-0.131	-0.141
		(0.088)	(0.117)
	Pop. density t-1	-0.004	-0.005
		(0.003)	(0.004)
	Trade/GDP t-1	-0.003	-0.006
		(0.005)	(0.006)
	Post Cold War	-0.906	-1.181
		(0.443)**	(0.538)**
	Prior regime: Autocracy t-1	-0.042	-0.669
	(*baseline cat.: democracy*)	(0.386)	(0.456)
	Prior regime: No authority t-1	-0.817	-0.103
	(*baseline cat.: democracy*)	(0.704)	(0.650)
NxT		5,355	1,057
Time polynomials		Yes	Yes
Regional dummies		No	No
Mil. regime t-1=1		No	Yes
Standard controls		Yes	Yes

Multinomial logit regression with cluster-robust standard errors. Column 1 unrestricted sample, base category: non-military regimes. Column 2 restricted sample on military regimes only, base category: indirect military rule. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A14: Regional rebellions and military regime democratization (additional variables)

	(1)	(2)	(3)	(4)	(5)	(6)
Previously experienced regional rebellion t-1	4.324			7.108		
	(2.125)**			(2.027)***		
Previously experienced center-seeking rebellion t-1	0.368			-1.079		
	(0.721)			(0.972)		
Directly following regional rebellion t-1		9.346			6.067	
		(4.266)**			(1.875)***	
Directly following center-seeking rebellion t-1		3.213			3.682	
		(1.896)*			(2.057)*	
Regional rebellion (decay) t-1			-0.451	-3.883	5.356	-97.412
			(1.296)	(1.544)**	(5.679)	(44.847)**
Center-seeking rebellion (decay) t-1			1.997	2.601	-1.628	-9.563
			(1.578)	(2.153)	(1.804)	(7.867)
Ethnic fractionalization t-1	5.538	7.816	4.679	4.846	9.420	
	(3.747)	(4.158)*	(3.292)	(3.489)	(4.908)*	
Oil rents (log) t-1	-0.805	-1.778	-0.596	-0.988	-2.048	-2.511
	(0.324)**	(0.786)**	(0.291)**	(0.352)***	(0.916)**	(1.526)*
Pop. density t-1	0.004	-0.002	0.002	0.004	-0.001	1.316
	(0.009)	(0.019)	(0.009)	(0.010)	(0.018)	(0.722)*
Trade/GDP t-1	-0.022	-0.028	-0.006	-0.030	-0.045	
	(0.017)	(0.010)***	(0.012)	(0.015)**	(0.019)**	
Post Cold War	-1.354	-3.513	-2.148	-0.858	-3.959	12.621
	(0.994)	(2.303)	(1.050)**	(0.764)	(2.762)	(7.103)*
NxT	1,029	900	1,029	1,029	900	471
Time polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Reginal dummies	Yes	Yes	Yes	Yes	Yes	Yes
Standard controls	Yes	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-5. Conditional logit model in column 6. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Controlling for diffusion effects

There is ample evidence that democratization is driven, in part, by regional diffusion effects (e.g. Elkins and Simmons 2005; Gleditsch and Ward 2006; Patel, Bunce, and Wolchik 2014). Similar dynamics can be imagined for the emergence of specific authoritarian regime types, such as military regimes. To ensure that our findings are not driven by diffusion, we control for the *percentage of military regimes in neighborhood* in our emergence regressions, and the *percentage of democratic regimes in neighborhood* for our democratization models. Neighborhood is defined as all contiguous states not separated by more than 24 miles of water. We use the Correlates of War Direct Contiguity dataset to construct these variables (Stinnett et al. 2002, v3.2). The results of this robustness test, shown in Table A15-Table A17, are substantively identical to those from our baseline models.

Table A15: Regional rebellions and military regime emergence (diffusion effects)

	(1)	(2)
Regional rebellion (decay) t-1	0.861	2.241
	(0.445)*	(0.986)**
Center-seeking rebellion (decay) t-1	-0.034	0.851
	(0.328)	(0.702)
% mil. regimes in neighborhood (contiguous) t-1	1.487	4.243
	(0.407)***	(1.209)***
GDP p.c. (log) t-1	-0.116	-0.781
	(0.298)	(0.745)
Population size (log) t-1	0.062	3.500
	(0.284)	(1.921)*
Polity t-1	-0.156	-0.412
	(0.036)***	(0.110)***
War (dummy) t-1	0.324	0.375
	(0.742)	(1.049)
Unrest (log) t-1	0.477	-0.031
	(0.170)***	(0.285)
Rel. fractionalization t-1	-1.470	
	(0.739)**	
Mil. capabilities (cube root) t-1	-0.031	-13.701
	(8.033)	(33.551)
Previous failures	0.084	-1.731
	(0.253)	(0.822)**
NxT	4,292	1,205
Time polynomials	Yes	Yes
Regional dummies	Yes	Yes

Pooled logit model in column 1. Conditional logit model in column 2. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A16: Regional rebellions and regime militarization (diffusion effects)

		(1)	(2)
Indirect Military	Duration of regional rebellion t-1	0.008	
		(0.041)	
	Duration of center-seeking rebellion t-1	-0.011	
		(0.031)	
	% mil. regimes in neighborhood (contiguous) t-1	1.143	
		(0.431)***	
Direct Military	Duration of regional rebellion t-1	0.116	0.116
		(0.027)***	(0.051)**
	Duration of center-seeking rebellion t-1	-0.142	-0.143
		(0.030)***	(0.052)***
	% mil. regimes in neighborhood (contiguous) t-1	1.257	0.053
		(0.476)***	(0.593)
NxT		5,355	1,057
Time polynomials		Yes	Yes
Regional dummies		No	No
Mil. regime t-1=1		No	Yes
Standard controls		Yes	Yes

Multinomial logit regression with cluster-robust standard errors. Column 1 unrestricted sample, base category: non-military regimes. Column 2 restricted sample on military regimes only, base category: indirect military rule. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A17: Regional rebellions and military regime democratization (diffusion effects)

	(1)	(2)	(3)	(4)	(5)	(6)
Previously experienced regional rebellion t-1	0.809			2.208		
	(2.069)			(2.460)		
Previously experienced center-seeking rebellion t-1	1.373			0.624		
	(0.999)			(1.595)		
Directly following regional rebellion t-1		0.240			1.854	
		(0.977)			(1.367)	
Directly following center-seeking rebellion t-1		0.115			-0.667	
		(1.577)			(1.461)	
Regional rebellion (decay) t-1			-2.640	-4.084	-1.583	-21.809
			(1.494)*	(2.212)*	(1.195)	(3.192)***
Center-seeking rebellion (decay) t-1			1.832	1.759	1.511	-1.682
			(1.060)*	(1.488)	(1.423)	(1.988)
% dem. regimes in neighborhood (contiguous) t-1	-1.074	1.307	-0.773	-1.037	0.134	10.784
	(1.919)	(2.009)	(1.839)	(1.877)	(2.455)	(3.794)***
NxT	1,146	1,003	1,146	1,146	1,003	457
Time polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes
Standard controls	Yes	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-5. Conditional logit model in column 6. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Alternative times specifications

We test the sensitivity of our baseline models to alternative time specifications in Table A18-Table A20 below. Specifically, we rerun the models using cubic splines, the log of time, and decade dummies with time polynomials. Our main findings remain substantively unchanged.

Table A18: Regional rebellions and military regime emergence (alternative time specifications)

	(1) *Logit*	(2) *Clogit*	(3) *Logit*	(4) *Clogit*	(5) *Logit*	(6) *Clogit*
Regional rebellion (decay) t-1	0.940	2.309	0.931	2.358	0.913	2.303
	(0.428)**	(1.004)**	(0.424)**	(0.910)***	(0.424)**	(1.197)*
Center-seeking rebellion (decay) t-1	0.324	1.147	0.205	1.049	0.392	0.916
	(0.346)	(0.749)	(0.349)	(0.674)	(0.349)	(0.846)
GDP p.c. (log) t-1	-0.005	-0.876	0.016	-0.244	-0.012	-1.017
	(0.269)	(0.836)	(0.257)	(0.750)	(0.273)	(0.953)
Population size (log) t-1	0.362	2.820	0.380	4.827	0.371	1.776
	(0.212)*	(2.020)	(0.211)*	(1.977)**	(0.223)*	(2.269)
Polity t-1	-0.152	-0.420	-0.161	-0.379	-0.135	-0.390
	(0.036)***	(0.096)***	(0.035)***	(0.083)***	(0.037)***	(0.104)***
War (dummy) t-1	0.379	0.368	0.319	0.363	0.306	0.197
	(0.728)	(1.084)	(0.753)	(0.994)	(0.745)	(0.911)
Unrest (log) t-1	0.544	-0.071	0.533	-0.064	0.507	-0.154
	(0.154)***	(0.308)	(0.153)***	(0.274)	(0.155)***	(0.299)
Rel. fractionalization t-1	-1.691		-1.689		-1.450	
	(0.747)**		(0.751)**		(0.752)*	
Mil. capabilities (cube root) t-1	-10.215	-19.317	-10.535	-32.703	-11.705	-22.140
	(5.011)**	(29.699)	(4.923)**	(33.536)	(5.220)**	(37.913)
Previous failures	0.165	-1.001	0.104	-1.863	0.463	-1.542
	(0.242)	(1.002)	(0.262)	(0.938)**	(0.293)	(0.942)
NxT	4,630	1,301	4,630	1,301	4,630	1,301
Cubic splines	Yes	Yes	No	No	No	No
Log of time	No	No	Yes	Yes	No	No
Decade dummies	No	No	No	No	Yes	Yes
Time polynomials	No	No	No	No	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1, 3, and 5. Conditional logit model in column 2, 4, and 6. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A19: Regional rebellions and regime militarization (alternative time specifications)

		(1)	(2)	(3)	(4)	(5)	(6)
Indirect Military	Duration of regional rebellion t-1	0.019	0.015	0.021			
		(0.046)	(0.042)	(0.036)			
	Duration of center-seeking rebellion t-1	-0.020	-0.019	-0.018			
		(0.033)	(0.031)	(0.029)			
Direct Military	Duration of regional rebellion t-1	0.119	0.120	0.112	0.116	0.116	0.098
		(0.032)***	(0.025)***	(0.028)***	(0.050)**	(0.050)**	(0.037)***
	Duration of center-seeking rebellion t-1	-0.142	-0.148	-0.150	-0.144	-0.144	-0.142
		(0.029)***	(0.029)***	(0.027)***	(0.051)***	(0.050)***	(0.041)***
NxT		6,052	6,052	6,052	1,200	1,200	1,200
Cubic splines		Yes	No	No	Yes	No	No
Log of time		No	Yes	No	No	Yes	No
Decade dummies		No	No	Yes	No	No	Yes
Time polynomials		No	No	Yes	No	No	Yes
Regional dummies		No	No	No	No	No	No
Mil. regime t-1=1		No	No	No	Yes	Yes	Yes
Standard controls		Yes	Yes	Yes	Yes	Yes	Yes

Multinomial logit regression with cluster-robust standard errors. Columns 1-3 unrestricted sample, base category: non-military regimes.
Columns 4-6 restricted sample on military regimes only, base category: indirect military rule. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A20: Regional rebellions and military regime democratization (alternative time specifications)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Logit	*Logit*	*Clogit*	*Logit*	*Logit*	*Clogit*	*Logit*	*Logit*	*Clogit*
Prev. exp. regional rebellion t-1	2.485			1.486			2.701		
	(1.657)			(1.587)			(1.996)		
Prev. exp. center-seeking rebellion t-1	-0.106			-0.029			0.124		
	(1.302)			(1.446)			(0.984)		
Dir. following regional rebellion t-1		2.125			2.184			2.588	
		(0.946)**			(1.210)*			(1.401)*	
Dir. following center-seeking rebellion t-1		-0.354			-0.420			0.284	
		(1.149)			(1.205)			(1.381)	
Regional rebellion (decay) t-1	-3.963	-1.387	-21.754	-3.437	-1.524	-25.277	-5.335	-3.632	-24.155
	(1.710)**	(0.948)	(3.244)***	(1.872)*	(1.071)	(232.768)	(2.854)*	(1.726)**	(8.595)***
Center-seeking rebellion (decay) t-1	1.924	1.305	-0.142	1.866	1.672	1.378	2.609	2.192	2.146
	(1.490)	(1.128)	(1.363)	(1.546)	(1.137)	(1.157)	(1.664)	(1.312)*	(2.299)
NxT	1,166	1,023	471	1,166	1,023	471	1,166	1,023	478
Cubic splines	Yes	Yes	Yes	No	No	No	No	No	No
Log of time	No	No	No	Yes	Yes	Yes	No	No	No
Decade dummies	No	No	No	No	No	No	Yes	Yes	Yes
Time polynomials	No	No	No	No	No	No	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Standard controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-2, 4-5, and 7-9. Conditional logit model in column 3, 6, and9. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Alternative measure for military regimes

The robustness tests below replicate our baseline models using an indicator of military regimes based on Geddes et al. (2014). Importantly, we depart from her schema in two ways. First, when a regime is classified by her as a hybrid regime, we recode the regime according to Lai and Slater (2006), who classify authoritarian regimes as one of four types: Strongman, Junta, Bossism, or Machine. We treat the first two types as instances of military regimes and the latter two types as party regimes. We make this departure from Geddes because her typology measures variation in regime type solely by how "access to office and the fruits of office" are allocated; that is, by military committee, party committee, or a single individual (Geddes 1999b, 121–122). Yet, authoritarian regimes should be differentiated by their executory powers and not just by their decision-making procedures. Doing so substantially reduces hybridization in the classification of all regime types.

Second, and by similar reasoning, we recode regimes classified as *purely personalist* by Geddes as *military* regimes, since almost all cases of personalist regimes rely on the military to maintain power and execute policy. Where Geddes sees a strong party apparatus accompanying personal rule, she classifies it as a party-personalist hybrid. This lends added confidence that Geddes' purely personalist regimes lack a ruling party that rivals the power of the military.

The results of this robustness test are displayed in Table A21 and Table A22. Regarding military regime emergence, the findings using Geddes et al.'s data are nearly identical to our baseline model, both in the pooled and in the fixed effects regression. As for democratization, the results are much weaker using the alternative measure. While the coefficient of *Regional rebellion (decay)* is negative throughout, it only reaches levels of statistical significance in Table A22, column 4. This

underlines the fact that the results for military regime democratization should be viewed as suggestive, rather than conclusive.

Table A21: Regional rebellions and military regime emergence (GWF data)

	(1)	(2)	(3)	(4)	(5)
Regional rebellion (decay) t-1	1.029	1.023	0.838	0.937	1.944
	(0.430)**	(0.429)**	(0.450)*	(0.444)**	(0.757)**
Center-seeking rebellion (decay) t-1	0.701	0.699	0.651	0.710	0.857
	(0.316)**	(0.317)**	(0.325)**	(0.323)**	(0.567)
GDP p.c. (log) t-1	-0.431	-0.435	-0.606	-0.451	-2.674
	(0.201)**	(0.200)**	(0.237)**	(0.285)	(1.331)**
Population size (log) t-1	0.043	0.050	-0.108	0.187	2.928
	(0.134)	(0.134)	(0.148)	(0.330)	(1.800)
Polity t-1	-0.131	-0.131	-0.147	-0.146	-0.316
	(0.026)***	(0.026)***	(0.028)***	(0.029)***	(0.050)***
Previous failures	0.008	0.008	0.007	-0.004	-0.157
	(0.014)	(0.014)	(0.016)	(0.017)	(0.078)**
War (dummy) t-1		-0.339	-0.265	-0.128	0.444
		(0.755)	(0.743)	(0.753)	(1.056)
Unrest (log) t-1			0.780	0.806	0.602
			(0.193)***	(0.184)***	(0.253)**
Rel. fractionalization t-1			-0.633	-0.506	
			(0.804)	(0.831)	
Mil. capabilities (cube root) t-1				-7.806	-9.579
				(7.602)	(41.881)
NxT	4,382	4,382	4,293	4,103	1,183
Time polynomials	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-4. Conditional logit model in column 5. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A22: Regional rebellions and military regime democratization (GWF data)

	(1)	(2)	(3)	(4)	(5)	(6)
Previously experienced regional rebellion t-1	0.333			0.723		
	(0.789)			(0.803)		
Previously experienced center-seeking rebellion t-1	-0.356			-0.021		
	(0.489)			(0.437)		
Directly following regional rebellion t-1		-1.606			-1.701	
		(1.631)			(1.706)	
Directly following center-seeking rebellion t-1		-0.838			-0.502	
		(0.607)			(0.556)	
Regional rebellion (decay) t-1			-0.569	-0.953	-0.926	-0.435
			(0.541)	(0.578)*	(0.771)	(0.929)
Center-seeking rebellion (decay) t-1			-0.980	-0.992	-0.658	-0.826
			(0.636)	(0.622)	(0.597)	(1.649)
NxT	1,337	951	1,337	1,337	951	548
Time polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes
Standard controls	Yes	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-5. Conditional logit model in column 6. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Alternative half-lives for our decaying variables

To recall, our main variables measuring exposure to rebellions – regional and center-seeking – are decaying variables with a half-life of one year, meaning that half of the effect decays after one year. To see if our findings are driven by this particular choice of half-life, we rerun our baseline models with alternative half-lives of two, three, and five years respectively. The results, displayed in Table A23-Table A28, are substantively identical to our baseline models, albeit slightly weaker in the case of military regime democratization with a half-life of five years.

Table A23: Regional rebellions and military regime emergence (half-life 2)

	(1)	(2)	(3)	(4)	(5)
Regional rebellion (HL2, decay) t-1	1.004	1.014	0.898	0.876	2.283
	(0.436)**	(0.432)**	(0.423)**	(0.409)**	(1.027)**
Center-seeking rebellion (HL2, decay) t-1	0.372	0.374	0.293	0.261	1.348
	(0.371)	(0.371)	(0.356)	(0.360)	(0.794)*
GDP p.c. (log) t-1	-0.151	-0.152	-0.166	0.017	-0.774
	(0.238)	(0.237)	(0.233)	(0.264)	(0.890)
Population size (log) t-1	0.038	0.041	-0.011	0.367	2.776
	(0.129)	(0.126)	(0.125)	(0.216)*	(2.113)
Polity t-1	-0.127	-0.128	-0.142	-0.154	-0.408
	(0.031)***	(0.031)***	(0.032)***	(0.035)***	(0.096)***
Previous failures	0.195	0.196	0.165	0.126	-1.136
	(0.215)	(0.216)	(0.238)	(0.251)	(1.003)
War (dummy) t-1		-0.143	0.039	0.370	0.337
		(0.770)	(0.749)	(0.742)	(1.066)
Unrest (log) t-1			0.482	0.516	-0.098
			(0.151)***	(0.155)***	(0.310)
Rel. fractionalization t-1			-1.572	-1.685	
			(0.694)**	(0.748)**	
Mil. capabilities (cube root) t-1				-10.185	-21.722
				(5.012)**	(33.329)
NxT	4,726	4,726	4,631	4,630	1,301
Time polynomials	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-4. Conditional logit model in column 5. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A24: Regional rebellions and military regime democratization (half-life 2)

	(1)	(2)	(3)	(4)	(5)	(6)
Previously experienced regional rebellion t-1	0.759			2.662		
	(1.190)			(2.030)		
Previously experienced center-seeking rebellion t-1	0.844			-0.482		
	(0.861)			(1.809)		
Directly following regional rebellion t-1		0.650			1.847	
		(0.879)			(2.149)	
Directly following center-seeking rebellion t-1		0.029			-0.923	
		(1.483)			(1.293)	
Regional rebellion (HL2, decay) t-1			-2.758	-4.340	-1.436	-13.441
			(1.524)*	(1.951)**	(2.053)	(4.607)***
Center-seeking rebellion (HL2, decay) t-1			1.866	2.460	1.955	1.090
			(0.989)*	(1.785)	(1.086)*	(2.333)
NxT	1,166	1,023	1,166	1,166	1,023	471
Time polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes
Standard controls	Yes	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-5. Conditional logit model in column 6. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A25: Regional rebellions and military regime emergence (half-life 3)

	(1)	(2)	(3)	(4)	(5)
Regional rebellion (HL3, decay) t-1	0.974	0.983	0.869	0.859	2.350
	(0.430)**	(0.425)**	(0.415)**	(0.403)**	(1.063)**
Center-seeking rebellion (HL3, decay) t-1	0.363	0.366	0.293	0.259	1.474
	(0.375)	(0.375)	(0.362)	(0.365)	(0.833)*
GDP p.c. (log) t-1	-0.146	-0.147	-0.161	0.023	-0.723
	(0.238)	(0.237)	(0.233)	(0.265)	(0.914)
Population size (log) t-1	0.038	0.040	-0.012	0.368	2.832
	(0.130)	(0.127)	(0.126)	(0.216)*	(2.138)
Polity t-1	-0.128	-0.128	-0.143	-0.154	-0.409
	(0.031)***	(0.031)***	(0.032)***	(0.035)***	(0.096)***
Previous failures	0.190	0.190	0.160	0.121	-1.177
	(0.217)	(0.217)	(0.239)	(0.252)	(1.010)
War (dummy) t-1		-0.137	0.038	0.369	0.266
		(0.763)	(0.746)	(0.740)	(1.079)
Unrest (log) t-1			0.485	0.518	-0.099
			(0.152)***	(0.155)***	(0.315)
Rel. fractionalization t-1			-1.568	-1.680	
			(0.695)**	(0.750)**	
Mil. capabilities (cube root) t-1				-10.285	-23.936
				(5.034)**	(34.249)
NxT	4,726	4,726	4,631	4,630	1,301
Time polynomials	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-4. Conditional logit model in column 5. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A26: Regional rebellions and military regime democratization (half-life 3)

	(1)	(2)	(3)	(4)	(5)	(6)
Previously experienced regional rebellion t-1	0.759			3.107		
	(1.190)			(2.096)		
Previously experienced center-seeking rebellion t-1	0.844			-0.769		
	(0.861)			(2.207)		
Directly following regional rebellion t-1		0.650			0.931	
		(0.879)			(2.894)	
Directly following center-seeking rebellion t-1		0.029			-0.961	
		(1.483)			(1.290)	
Regional rebellion (HL3, decay) t-1			-2.791	-4.838	-0.529	-14.690
			(1.565)*	(2.045)**	(2.733)	(15.613)
Center-seeking rebellion (HL3, decay) t-1			1.872	2.758	2.090	2.548
			(0.992)*	(2.116)	(1.124)*	(2.387)
NxT	1,166	1,023	1,166	1,166	1,023	471
Time polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes
Standard controls	Yes	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-5. Conditional logit model in column 6. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A27: Regional rebellions and military regime emergence (half-life 5)

	(1)	(2)	(3)	(4)	(5)
Regional rebellion (HL5, decay) t-1	0.930	0.938	0.826	0.837	2.381
	(0.419)**	(0.414)**	(0.404)**	(0.395)**	(1.119)**
Center-seeking rebellion (HL5, decay) t-1	0.338	0.341	0.281	0.246	1.612
	(0.382)	(0.382)	(0.372)	(0.375)	(0.894)*
GDP p.c. (log) t-1	-0.141	-0.142	-0.157	0.031	-0.659
	(0.237)	(0.237)	(0.233)	(0.265)	(0.945)
Population size (log) t-1	0.039	0.041	-0.013	0.373	2.862
	(0.130)	(0.128)	(0.127)	(0.217)*	(2.172)
Polity t-1	-0.128	-0.128	-0.143	-0.155	-0.410
	(0.031)***	(0.031)***	(0.032)***	(0.035)***	(0.096)***
Previous failures	0.182	0.183	0.151	0.114	-1.214
	(0.220)	(0.220)	(0.242)	(0.255)	(1.011)
War (dummy) t-1		-0.130	0.034	0.367	0.156
		(0.751)	(0.741)	(0.736)	(1.087)
Unrest (log) t-1			0.488	0.520	-0.098
			(0.152)***	(0.155)***	(0.322)
Rel. fractionalization t-1			-1.559	-1.671	
			(0.696)**	(0.754)**	
Mil. capabilities (cube root) t-1				-10.482	-26.702
				(5.076)**	(35.579)
NxT	4,726	4,726	4,631	4,630	1,301
Time polynomials	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-4. Conditional logit model in column 5. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A28: Regional rebellions and military regime democratization (half-life 5)

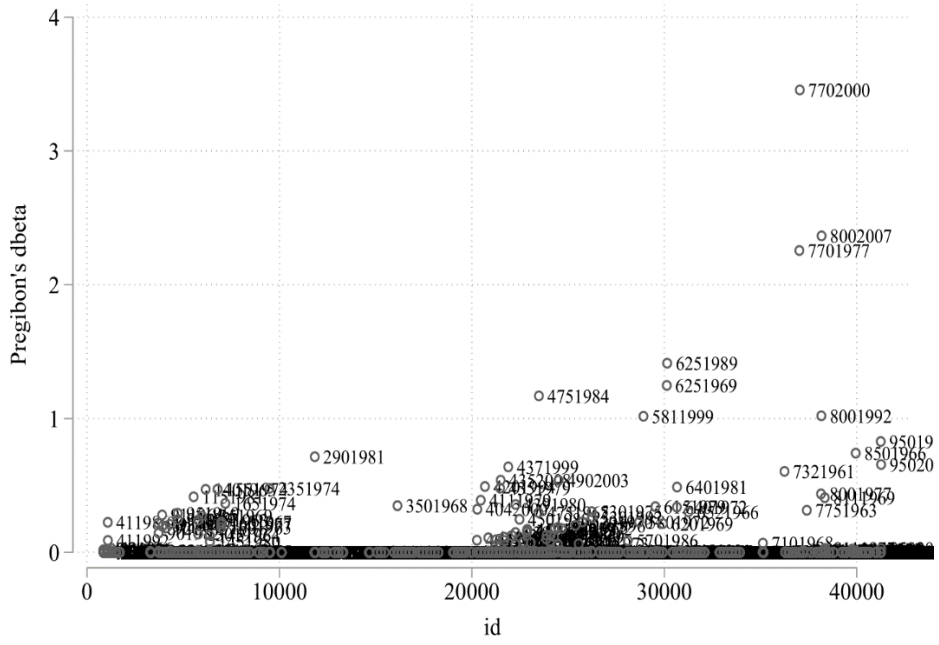
	(1)	(2)	(3)	(4)	(5)	(6)
Previously experienced regional rebellion t-1	0.759			3.951		
	(1.190)			(2.284)*		
Previously experienced center-seeking rebellion t-1	0.844			-1.167		
	(0.861)			(2.944)		
Directly following regional rebellion t-1		0.650			0.011	
		(0.879)			(3.018)	
Directly following center-seeking rebellion t-1		0.029			-0.948	
		(1.483)			(1.296)	
Regional rebellion (HL5, decay) t-1			-2.673	-5.794	0.341	-35.328
			(1.632)	(2.366)**	(2.805)	(36.325)
Center-seeking rebellion (HL5, decay) t-1			1.767	3.134	2.185	4.311
			(0.985)*	(2.832)	(1.198)*	(3.421)
NxT	1,166	1,023	1,166	1,166	1,023	471
Time polynomials	Yes	Yes	Yes	Yes	Yes	Yes
Regional dummies	Yes	Yes	Yes	Yes	Yes	Yes
Standard controls	Yes	Yes	Yes	Yes	Yes	Yes

Pooled logit model in columns 1-5. Conditional logit model in column 6. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Influential observations

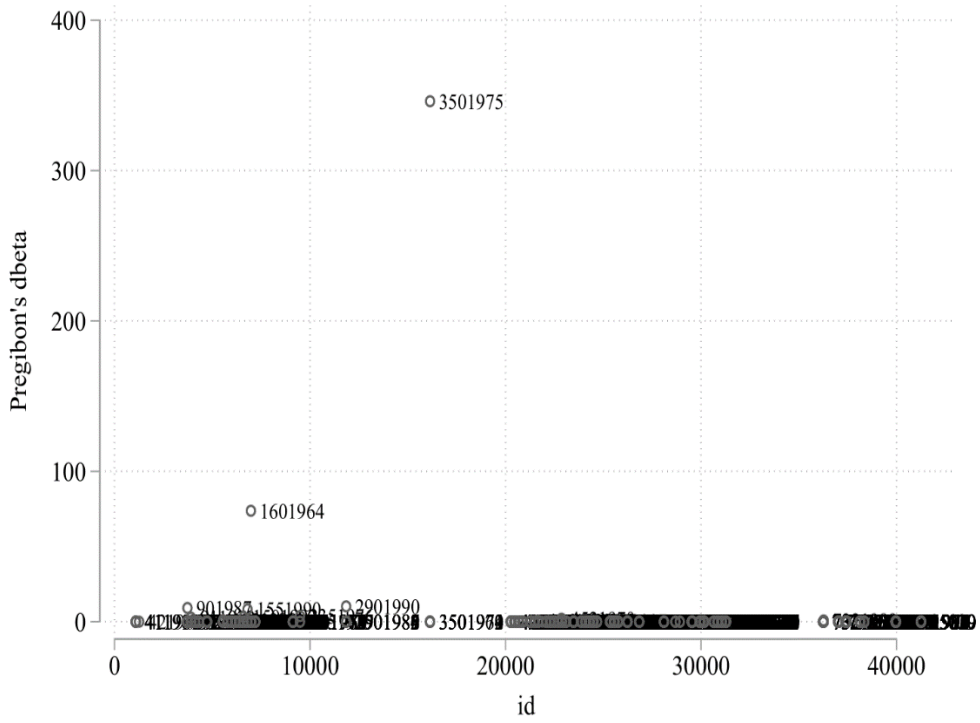
To ensure that our results are not solely driven by a few influential observations, we rerun our regressions while dropping observations with particularly high leverage. To identify these observations, we are using Pregibon's dbeta which provides information on the influence on parameter estimates of each individual observation. There is no critical value for this measure and influential observations have to be identified using eyeballing. In light of Figure A7, Pakistan (cowcode770) and Thailand (cowcode 800) seem to contain influential observations. Figure A8 highlights Argentina (cowcode 160) and Greece (cowcode 350) as influential observations. We therefore remove these countries from the regression and rerun our baseline models. The results shown in Table A29 and Table A30 are substantively identical to our core findings, albeit slightly weaker in both cases. We thus conclude that our results are not a pure artefact of a few high-leverage observations.

Figure A7: Influential observations for military regime emergence



Note: Labels represent cowcode-years.

Figure A8: Influential observations for military regime democratization



Note: Labels represent cowcode-years.

Table A29: Regional rebellions and military regime emergence (w/o influential observations)

	(1)
Regional rebellion (decay) t-1	0.793
	(0.451)*
Center-seeking rebellion (decay) t-1	0.444
	(0.363)
GDP p.c. (log) t-1	-0.169
	(0.249)
Population size (log) t-1	0.330
	(0.224)
Polity t-1	-0.144
	(0.034)***
War (dummy) t-1	0.163
	(0.883)
Unrest (log) t-1	0.562
	(0.162)***
Rel. fractionalization t-1	-1.585
	(0.755)**
Mil. capabilities (cube root) t-1	-10.092
	(5.103)**
Previous failures	-0.089
	(0.250)
NxT	4,368
Time polynomials	Yes
Regional dummies	Yes

Pooled logit model replicating column 4, Table 1 in main article, without Pakistan and Thailand. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A30: Regional rebellions and military regime democratization (w/o influential observations)

	(1)
Previously experienced regional rebellion t-1	1.717
	(2.193)
Previously experienced center-seeking rebellion t-1	-1.823
	(1.117)
Regional rebellion (decay) t-1	-4.259
	(2.371)*
Center-seeking rebellion (decay) t-1	2.350
	(1.484)
GDP p.c. (log) t-1	0.011
	(0.854)
Population size (log) t-1	-0.075
	(1.059)
Polity t-1	1.034
	(0.218)***
Mil. capabilities (cube root) t-1	-4.079
	(20.354)
Unrest (log) t-1	0.156
	(0.668)
War (dummy) t-1	-3.257
	(1.705)*
Rel. fractionalization t-1	12.127
	(3.967)***
Number of previous regime failures	0.979
	(0.521)*
NxT	1,144
Time polynomials	Yes
Regional dummies	Yes

Pooled logit model replicating column 4, Table 4 in main article, without Argentina and Greece. Cluster-robust standard errors in parentheses. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

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